II. *An Account of the Book entituled* Commercium Epiſtolicum Collinii & aliorum, De Analyſi promota; *publiſhed by order of the* Royal-Society, *in relation to the Diſpute between Mr.* Leibnitz *and Dr.* Keill, *about the Right of Invention of the Method of* Fluxions, *by ſome call'd* the Differential Method.

SEveral Accounts having been publiſhed abroad of this *Commercium*, all of them very imperfect: It has been thought fit to publiſh the Account which follows.

This *Commercium* is compoſed of ſeveral ancient Letters and Papers, put together in order of Time, and either copied or tranſlated into Latin from ſuch Originals as are deſcribed in the Title of every Letter and Paper; a numerous Committee of the Royal-Society being appointed to examin the Sincerity of the Originals, and compare therewith the Copies taken from them. It relates to a general Method of reſolving finite Equations into infinite ones, and applying theſe Equations, both finite and infinite, to the Solution of Problems by the Method of Fluxions and Moments. We will firſt give an Account of that Part of the Method which conſiſts in reſolving finite Equations into infinite ones, and ſquaring curvilinear Figures thereby. By Infinite Equations are meant ſuch as involve a Series of Terms converging or approaching the Truth nearer and nearer *in infinitum*, ſo as at length to differ from the Truth leſs than by any given Quantity, and if continued *in infinitum*, to leave no Difference.

Dr.

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Dr. *Wallis* in his *Opus Arithmeticum* publiſhed *A.C.* 1657: *Cap.* 33. *Prop.* 68. reduced the Fraction $\frac{A}{1-R}$ by perpetual Diviſion into the Series $A + AR + AR^2 + AR^3 + AR^4 +$ *&c.*

Viſcount *Brounker* ſquared the Hyperbola by this Series $\frac{1}{1\times2} + \frac{1}{3\times4} + \frac{1}{5\times6} + \frac{1}{7\times8} +$ *&c.* that is by this, $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} +$ *&c.* conjoyning every two Terms into one. And the Quadrature was publiſhed in the *Philoſophical Tranſactions* for *April* 1668.

Mr. *Mercator* ſoon after publiſhed a Demonſtration of this Quadrature by the Diviſion of Dr. *Wallis*; and ſoon after that Mr. *James Gregory* publiſhed a Geometrical Demonſtration thereof. And theſe Books were a few Months after ſent by Mr. *John Collins* to Dr. *Barrow* at *Cambridge*, and by Dr. *Barrow* communicated to Mr. *Newton* (now Sir *Iſaac Newton*) in *June* 1669. Whereupon Dr *Barrow* mutually ſent to Mr. *Collins* a Tract of Mr. *Newton*'s entituled *Analyſis per æquationes numero terminorum infinitas.* And this is the firſt Piece publiſhed in the *Commercium*, and contains a general Method of doing in all Figures, what my Lord *Brounker* and Mr. *Mercator* did in the Hyperbola alone. Mr. *Mercator* lived above ten Years longer without proceeding further than to the ſingle Quadrature of the Hyperbola. The Progreſs made by Mr. *Newton* ſhews that he wanted not Mr. *Mercator*'s Aſſiſtance. However, for avoiding Diſputes, he ſuppoſes that my Lord *Brounker* invented, and Mr. *Mercator* demonſtrated, the Series for the Hyperbola ſome Years before they publiſhed it, and, by conſequence, before he found his general Method.

The aforeſaid Treatiſe of *Analyſis* Mr. *Newton*, in his Letter to Mr. *Oldenburgh*, dated *Octob.* 24. 1676. mentions in the following Manner. *Eo ipſo tempore quo* Mercatoris *Logarithmotechnia prodiit, communicatum eſt per amicum* D. Barrow (*tunc* Matheſeos *Profeſſorem* Cantab) *cum* D. Collinio *Compendium quoddam harum Serierum, in quo ſignificaveram Areas & Longitudines Curvarum omnium, & Solidorum ſuperficies & contenta ex* *datis rectis ; & vice verſa ex his datis rectas determinari poſſe: & methodum indicatam illuſtraveram diverſis ſeriebus.* Mr. *Collins* in the Years 1669, 1670, 1671 and 1672 gave notice of this Compendium to Mr *James Gregory* in *Scotland*, Mr. *Bertet* and Mr. *Vernon* then at *Paris*, Mr. *Alphonſus Borelli* in *Italy*, and Mr. *Strode*, Mr. *Townſend*, Mr. *Oldenburgh*, Mr. *Dary* and others in *England*, as appears by his Letters. And Mr. *Oldenburg* in a Letter, dated *Sept.* 14. 1669. and entred in the Letter-Book of the Royal-Society, gave notice of it to Mr. *Francis Sluſius* at *Liege*, and cited ſeveral Sentences out of it. And particularly Mr. *Collins* in a Letter to Mr. *James Gregory* dated *Novemb.* 25. 1669. ſpake thus of the Method contained in it. Barrovius *Provinciam ſuam publicè prælegendi remiſit cuidam nomine* Newtono Cantabrigienſi, *cujus tanquam viri acutiſſimo ingenio præditi in Præfatione Prælectionum Opticarum, meminit: quippe antequam ederetur* Mercatoris *Logarithmotechnia, eandem methodum adinvenerat, eamque ad omnes Curvas generaliter & ad Circulum diverſimode applicârat.* And in a Letter to Mr. *David Gregory* dated *Auguſt* 11. 1676. he mentions it in this manner. *Paucos poſt menſes quam editi ſunt hi Libri* [viz. *Mercatoris* Logarithmotechnia & Exercitationes Geometricæ *Gregorii*] *miſſi ſunt ad* Barrovium Cantabrigiæ. *Ille autem reſponſum dedit hanc infinitarum Serierum Doctrinam à* Newtono *biennium ante excogitatam fuiſſe quam ederetur* Mercatoris *Logarithmotechnia & generaliter omnibus figuris applicatam, ſimulque tranſmiſit D.* Newtoni *opus manuſcriptum.* The laſt of the ſaid two Books came out towards the End of the Year 1668, and Dr. *Barrow* ſent the ſaid Compendium to Mr. *Collins* in *July* following, as appears by three of Dr. *Barrow*'s Letters. And in a Letter to Mr. *Strode*, dated *July* 26. 1672, Mr. *Collins* wrote thus of it. *Exemplar ejus* [Logarithmotechniæ] *miſi* Barrovio Cantabrigiam, *qui quaſdam* Newtoni *chartas extemplò remiſit: E quibus & aliis quæ prius ab authore cum* Barrovio *communicata fuerant, patet illam methodum à dicto* Newtono *aliquot annis antea excogitatam & modo univer-*

ſali applicatam fuiſſe: Ita ut ejus ope, in quavis Figura Curvilinea propoſita, quæ una vel pluribus proprietatibus definitur, Quadratura vel Area dictæ figuræ, accurata ſi poſſibile ſit, ſin minus infinitè verò propinqua, Evolutio vel longitudo Lineæ Curvæ, Centrum gravitatis figuræ, Solida ejus rotatione genita & eorum ſuperficies; ſine ulla radicum extractione obtineri queant. Poſtquam intellexerat D. Gregorius *hanc methodum à D.* Mercatore *in Logarithmotechnia uſurpatam & Hyperbolæ quadrandæ adhibitam, quamque adauxerat ipſe* Gregorius, *jam univerſalem redditam eſſe, omnibuſque figuris applicatam; acri ſtudio eandem acquiſivit multumque in ea enodanda deſudavit. Uterque D.* Newtonus *&* Gregorius *in animo habet hanc methodum exornare: D.* Gregorius *autem D.* Newtonum *primum ejus inventorem anticipare haud integrum ducit.* And in another Letter written to Mr. *Oldenburgh* to be communicated to Mr. *Leibnitz,* and dated *June* 14 1676. Mr. *Collins* adds: *Hujus autem methodi ea eſt præſtantia ut cum tam late pateat ad nullam hæreat difficultatem.* Gregorium *autem alioſque in ea fuiſſe opinione arbitror, ut quicquid uſpiam antea de hac re innotuit, quaſi dubia diluculi lux fuit ſi cum meridiana claritate conferatur.*

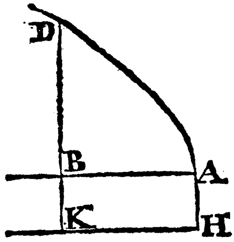
This Tract was firſt printed by Mr *William Jones,* being found by him among the Papers and in the Hand-writing of Mr. *John Collins,* and collated with the Original which he afterwards borrowed of Mr. *Newton.* It contains the above-mention'd general Method of *Analyſis,* teaching how to reſolve finite Equations into infinite ones, and how by the method of Moments to apply Equations both finite and infinite to the Solution of all Problems. It begins where Dr. *Wallis* left off, and founds the method of Quadratures upon three Rules.

Dr. *Wallis* publiſhed his *Arithmetica infinitorum* in the Year 1655, and by the 59*th* Propoſition of that Book, if the *Abſciſſa* of any curvilinear Figure be called x, and m and n be Numbers, and the Ordinates erected at right Angles be $x^{\frac{m}{n}}$, the Area of the Figure ſhall be $\frac{n}{m+n} x^{\frac{m+n}{n}}$. And this is aſſumed by Mr. *Newton* as the firſt Rule upon which he founds his Quadrature of Curves. Dr. *Wallis* demonſtrated this Propoſition by Steps in many particular Propoſitions, and then collected all the Propoſitions into One by a Table of the Caſes. Mr. *Newton* reduced all the Caſes into One, by a Dignity with an indefinite Index, and at the End of his Compendium demonſtrated it at once by his method of Moments, he being the firſt who introduced indefinite Indices of Dignities into the Operations of *Analyſis*.

By the 108*th* Propoſition of the ſaid *Arithmetica Infinitorum*, and by ſeveral other Propoſitions which follow it; if the Ordinate be compoſed of two or more Ordinates taken with their Signes + and —, the Area ſhall be compos'd of two or more Areas taken with their Signes + and — reſpectively. And this is aſſumed by Mr. *Newton* as the ſecond Rule upon which he founds his Method of Quadratures.

And the third Rule is to reduce Fractions and Radicals, and the affected Roots of Equations into converging Series, when the Quadrature does not otherwiſe ſucceed; and by the firſt and ſecond Rules to ſquare the Figures, whoſe Ordinates are the ſingle Terms of the Series. Mr. *Newton*, in his Letter to Mr. *Oldenburgh* dated *June* 13. 1676. and communicated to Mr. *Leibnitz*, taught how to reduce any Dignity of any Binominal into a converging Series, and how by that Series to ſquare the Curve, whoſe Ordinate is that Dignity. And being deſired by Mr. *Leibnitz* to explain the Original of this Theoreme, he replied in his Letter dated *Octob.* 24. 1676, that a little before the Plague (which raged in *London* in the Year 1665) upon reading the *Arithmetica Infinitorum* of Dr. *Wallis*, and conſidering how to interpole the Series x, $x - \frac{1}{3}x^3$, $x - \frac{2}{3}x^3 + \frac{1}{5}x^5$, $x - \frac{3}{3}x^3 + \frac{3}{5}x^5 - \frac{1}{7}x^7$, *&c.* he found the Area of a Circle to be $x - \frac{\frac{1}{2}x^3}{3} - \frac{\frac{1}{8}x^5}{5} - \frac{\frac{1}{16}x^7}{7} - \frac{\frac{5}{128}x^9}{9} -$ *&c.* And by purſuing the Method of Interpolation he found the Theoreme abovemention'd, and by means of this Theoreme he found the Reduction of Fractions and Surds into converging Series, by Diviſion and Extraction of Roots; and then proceeded to the Extraction of affected Roots. And theſe Reductions are his third Rule.

When Mr. *Newton* had in this Compendium explained theſe three Rules, and illuſtrated them with various Examples, he laid down the Idea of deducing the Area from the Ordinate, by conſidering the Area as a Quantity, growing or increaſing by continual Flux, in proportion to the Length of the Ordinate, ſuppoſing the Abſciſſa to increaſe uniformly in proportion to Time And from the Moments of Time he gave the Name of Moments to the momentaneous Increaſes, or infinitely ſmall Parts of the Abſciſſa and Area, generated in Moments of Time. The Moment of a Line he called a Point, in the Senſe of *Cavallerius*, tho' it be not a geometrical Point, but a Line infinitely ſhort, and the Moment of an Area or Superficies he called a Line, in the Senſe of *Cavallerius*, tho' it be not a geometrical Line, but a Superficies infinitely narrow. And when he conſider'd the Ordinate as the Moment of the Area, he underſtood by it the Rectangles under the geometrical Ordinate and a Moment of the Abſciſſa, tho' that Moment be not always expreſſed. *Sit ABD*, ſaith he, *Curva quævis, & AHKB rectangulum cujus latus AH* vel *KB eſt unitas. Et cogita rectam DBK uniformiter ab AH motam areas ABD & AK deſcribere; & quod* [recta] *BK (1) ſit momentum quo* [area] *AK (x), &* [recta] *BD (y) momentum quo* [area curvilinea] *ABD gradatim augetur; & quod ex momento BD perpetim dato poſſis, per præcedentes* [tres] *Regulas, aream ABD ipſo deſcriptam inveſtigare, ſive cum area AK (x) momento* 1 *deſcripta conferre.* This is his Idea of the Work in ſquaring of Curves, and how he applies this to other Problems, he expreſſes in the next Words. *Jam qua ratione*, ſaith he, *ſuperficies ABD ex momento ſuo perpetim*



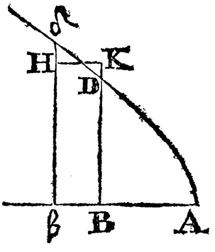
petim dato per præcedentes [tres] *Regulas elicitur, eâdem qualibet alia quantitas ex momento ſuo ſic dato elicietur. Exemplo res fiet clarior.* And after ſome Examples he adds his Method of Regreſſion from the Area, Arc, or ſolid Content, to the Abſciſſa; and ſhews how the ſame Method extends to Mechanical Curves, for determining their Ordinates, Tangents, Areas, Lengths, *&c.* And that by aſſuming any Equation expreſſing the Relation between the Area and Abſciſſa of a Curve, you may find the Ordinate by this Method. And this is the Foundation of the Method of Fluxions and Moments, which Mr. *Newton* in his Letter dated *Octob.* 24, 1676 comprehended in this Sentence. *Data æquatione quotcunque fluentes quantitates involvente, invenire Fluxiones; & vice verſa.*

In this Compendium Mr. *Newton* repreſents the uniform Fluxion of Time, or of any Exponent of Time by an Unit; the Moment of Time or of its Exponent by the Letter *o*; the Fluxions of other Quantities by any other Symbols; the Moments of thoſe Quantities by the Rectangles under thoſe Symbols and the Letter *o*; and the Area of a Curve by the Ordinate incloſed in a Square, the Area being put for a Fluent and the Ordinate for its Fluxion. When he is demonſtrating any Propoſition he uſes the Letter *o* for a finite Moment of Time, or of its Exponent, or of any Quantity flowing uniformly, and performs the whole Calculation by the Geometry of the Ancients in finite Figures or Schemes without any Approximation: and ſo ſoon as the Calculation is at an End, and the Equation is reduced, he ſuppoſes that the Moment *o* decreaſes *in infinitum* and vaniſhes. But when he is not demonſtrating but only inveſtigating a Propoſition, for making Diſpatch he ſuppoſes the Moment *o* to be infinitely little, and forbears to write it down, and uſes all manner of Approximations which he conceives will produce no Error in the Concluſion. An Example of the firſt kind you have in the End of this Compendium, in demonſtrating the firſt of the three Rules laid down in the Beginning of the Book.

Examples of the ſecond kind you have in the ſame Compendium, in finding the Length of Curve Lines *p.* 15. and in finding the Ordinates, Areas and Lengths of Mechanical Curves *p.* 18. 19. And he tells you, that by the ſame Method, Tangents may be drawn to mechanical Curves *p.* 19. And in his Letter of *Decemb.* 10. 1672. he adds, that Problems about the Curvature of Curves Geometrical or Mechanical are reſolv'd by the ſame Method. Whence its manifeſt, that he had then extended the Method to the ſecond and third Moments. For when the Areas of Curves are conſidered as Fluents (as is uſual in this *Analyſis*) the Ordinates expreſs the firſt Fluxions, the Tangents are given by the ſecond Fluxions, and the Curvatures by the third, And even in this *Analyſis* *p.* 16. where Mr. *Newton* ſaith, *Momentum eſt ſuperficies cum de ſolidis, & Linea cum de ſuperficiebus, & Punctum cum de lineis agitur*, it is all one as if he had ſaid, that when Solids are conſidered as Fluents, their Moments are Superficies, and the Moments of thoſe Moments (or ſecond Moments) are Lines, and the Moments of thoſe Moments (or third Moments) are Points, in the Senſe of *Cavallerius.* And in his *Principia Philoſophiæ*, where he frequently conſiders Lines as Fluents deſcribed by Points, whoſe Velocities increaſe or decreaſe, the Velocities are the firſt Fluxions, and their Increaſe the ſecond. And the Probleme, *Data æquatione fluentes quantitates involvente fluxiones invenire & vice verſa*, extends to all the Fluxions, as is manifeſt by the Examples of the Solution thereof, publiſhed by Dr. *Wallis Tom.* 2. *p* 391, 392, 396. And in *Lib.* II. *Princip. Prop.* XIV. he calls the ſecond Difference the Difference of Moments.

Now that you may know what kind of Calculation Mr. *Newton* uſed in, or before the Year 1669. when he wrote this Compendium of his *Analyſis*, I will here ſet down his Demonſtration of the firſt Rule abovementioned. *Sit*

Curvæ



Sit Curvæ alicujus A D δ *Baſis* A B $= x$, *perpendiculariter applicata* B D $= y$, *&* *area* A B D $= z$, *ut prius. Item ſit* $B\beta = o$, $BK = v$, *& Rectangulum* BβHK (ov) *æquale ſpatio* BβδD. *Eſt ergo* $A\beta = x + o$, *&* $A\delta\beta = z + ov$, *His præmiſſis, ex relatione inter* x *&* z *ad arbitrium aſſumpta, quæro* y *ut ſequitur.*

Pro lubitu ſumatur [æquatio] $\frac{2}{3}x^{\frac{3}{2}} = z$, *ſive* $\frac{4}{9}x^3 = zz$. *Tum* $x + o$ $(A\beta)$ *pro* x, *&* $z + ov$ $(A\delta\beta)$ *pro* z *ſubſtitutis, prodibit* $\frac{4}{9}$ *in* $x^3 + 3x^2o + 3xo^2 + o^3 =$ (*ex natura Curvæ*) $z^2 + {}$ $+ 2zov + o^2v^2$. *Et ſublatis* $\frac{4}{9}x^3$ *&* zz *æqualibus, reliquiſque per* o *diviſis, reſtat* $\frac{4}{9}$ *in* $3x^2 + 3xo + o^2 = 2zv + ov^2$. *Si jam ſupponamus* Bβ *in infinitum diminui & evaneſcere, ſive* o *eſſe nihil, erunt* v *&* y *æquales, & termini per* o *multiplicati evaneſcent; ideoque reſtabit* $\frac{4}{9} \times 3xx = 2zv$, *ſive* $\frac{2}{3}xx (= zy) = \frac{2}{3}x^{\frac{3}{2}}y$, *ſive* $x^{\frac{1}{2}} (= \frac{x^2}{x^{\frac{3}{2}}}) = y$. *Quare è contra, ſi* $x^{\frac{1}{2}} = y$, *erit* $\frac{2}{3}x^{\frac{3}{2}} = z$.

Vel generaliter, Si $\frac{n}{m+n} \times a x^{\frac{m+n}{n}} = z$; *ſive ponendo* $\frac{na}{m+n} = c$, *&* $m + n = p$, *Si* $c x^{\frac{p}{n}} = z$, *ſive* $c^n x^p = z^n$: *Tum* $x+o$ *pro* x, *&* $z + ov$ *ſive (quod perinde eſt)* $z + oy$ *pro* z *ſubſtitutis, prodit* c^n *in* $x^p + pox^{p-1}$ &c. $= z^n + noyz^{n-1}$ &c. *reliquis nempe* [Serierum] *terminis, qui tandem evaneſcerent, omiſſis. Jam ſublatis* c^nx^p *&* z^n *æqualibus, reliquiſque per* o *diviſis, reſtat* $c^n p x^{p-1} = nyz^{n-1} (= \frac{nyz^n}{z} = \frac{nyc^nx^p}{cx^{\frac{p}{n}}})$ *ſive dividendo per* c^nx^p, *erit* $px^{-1} = \frac{ny}{cx^{\frac{p}{n}}}$ *ſive* $pcx^{\frac{p-n}{n}} = ny$; *vel reſtituendo* $\frac{na}{m+n}$ *pro* c *&* $m + n$ *pro* p, *hoc eſt* m *pro* $p - n$, *&* na

pro pc, *fiet* $ax^{\frac{m}{n}}=y$. *Quare è contra, ſi* $ax^{\frac{m}{n}}=y$ *erit* $\frac{n}{m+n}ax^{\frac{m+n}{n}}$ $=z$ Q. E. D.

By the ſame way of working the ſecond Rule may be alſo demonſtrated. And if any Equation whatever be aſſumed expreſſing the Relation between the Abſciſſa and Area of a Curve, the Ordinate may be found in the ſame manner, as is mentioned in the next Words of the *Analyſis.* And if this Ordinate drawn into an Unit be put for the Area of a new Curve, the Ordinate of this new Curve may be found by the ſame Method: And ſo on perpetually. And theſe Ordinates repreſent the firſt, ſecond, third, fourth and following Fluxions of the firſt Area.

This was Mr. *Newton*'s Way of working in thoſe Days, when he wrote this Compendium of his *Analyſis.* And the ſame Way of working he uſed in his Book of Quadratures, and ſtill uſes to this Day.

Among the Examples with which he illuſtrates the Method of Series and Moments ſet down in this Compendium, are theſe. Let the Radius of a Circle be 1, and the Arc z, and the Sine x, the Equations for finding the Arc whoſe Sine is given, and the Sine whoſe Arc is given, will be

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&c.$$
$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \&c.$$

Mr. *Collins* gave Mr. *Gregory* notice of this Method in Autumn 1669, and Mr. *Gregory*, by the Help of one of Mr. *Newton*'s Series, after a Year's Study, found the Method in *December* 1670; and two Months after, in a Letter dated *Feb.* 15. 1671. ſent ſeveral Theorems, found thereby, to Mr. *Collins*, with leave to communicate them freely. And Mr. *Collins* was very free in communicating what he had received both from Mr. *Newton* and from Mr. *Gregory*, as appears by his Letters printed in the *Commercium.* Amongſt the Series which Mr *Gregory* ſent in the ſaid Letter, were theſe two. Let the Radius of a Circle be *r*, the Arc *a*, and the Tangent *t*, the Equations for finding the Arc whoſe Tangent is given, and the Tangent whoſe Arc is given, will be theſe.

$$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.$$

$$t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \&c.$$

In this Year (1671) Mr. *Leibnitz* publiſhed two Tracts at *London*, the One dedicated to the Royal-Society, the Other dedicated to the Academy of Sciences at *Paris*; and in the Dedication of the Firſt he mentioned his Correſpondence with Mr. *Oldenburgh*.

In *February* 167$\frac{2}{3}$ meeting Dr. *Pell* at Mr. *Boyle*'s, he pretended to the differential Method of *Mouton*. And notwithſtanding that he was ſhewn by Dr. *Pell* that it was *Mouton*'s Method, he perſiſted in maintaining it to be his own Invention, by reaſon that he had found it himſelf without knowing what *Mouton* had done before, and had much improved it.

When one of Mr. *Newton*'s Series was ſent to Mr. *Gregory*, he tried to deduce it from his own Series combined together, as he mentions in his Letter dated *December* 19. 1670. And by ſome ſuch Method Mr *Leibnitz*, before he left *London*, ſeems to have found the Sum of a Series of Fractions decreaſing *in Infinitum*, whoſe Numerator is a given Number and Denominators are triangular or pyramidal or triangulo-triangular Numbers, *&c.* See the Myſtery! From the Series $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \&c.$ ſubduct all the Terms but the firſt (*viz* $\frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5}$ *&c*) and there will remain $1 = 1 - \frac{1}{2} + \frac{1}{2} - \frac{1}{3} + \frac{1}{3} - \frac{1}{4} + \frac{1}{4} - \frac{1}{5} + \frac{1}{5}\ \&c) = \frac{1}{1\times2} + \frac{1}{2\times3} + \frac{1}{3\times4} + \frac{1}{4\times5} + \&c.$ And from this Series take all the Terms but the firſt, and there will remain $\frac{1}{2} = \frac{2}{1\times2\times3} + \frac{2}{2\times3\times4} + \frac{2}{3\times4\times5} + \frac{2}{4\times5\times6} + \&c.$ And from the firſt Series take all the Terms but the two firſt, and there will remain $\frac{3}{2} = \frac{2}{1\times3} + \frac{2}{2\times4} + \frac{2}{3\times5} + \frac{2}{4\times6} + \&c.$

In the End of *February* or beginning of *March* $167\frac{2}{3}$. Mr. *Leibnitz* went from *London* to *Paris*, and continuing his Correſpondence with Mr *Oldenburg* and Mr. *Collin* , wrote in *July* 1674. that he had a wonderful Theoreme, which gave the Area of a Circle or any Sector thereof exactly in a Series of rational Numbers; and in *October* following, that he had found the Circumference of a Circle in a Series of very ſimple Numbers, and that by the ſame Method (ſo he calls the ſaid Theoreme) any Arc whoſe Sine was given might be found in a like Series, though the Proportion to the whole Circumference be not known. His Theoreme therefore was for finding any Sector or Arc whoſe Sine was given. If the Proportion of the Arc to the whole Circumference was not known, the Theoreme or Method gave him only the Arc; if it was known it gave him alſo the whole Circumference: and therefore it was the firſt of Mr. *Newton*'s two Theoremes above-mention'd. But the Demonſtration of this Theoreme Mr *Leibnitz* wanted. For in his Letter of *May* 12. 1676. he deſired Mr. *Oldenburgh* to procure the Demonſtration from Mr *Collins*, meaning the Method by which Mr. *Newton* had invented it.

In a Letter compos'd by Mr. *Collins* and dated *April* 15. 1675. Mr. *Oldenburgh* ſent to Mr. *Leibnitz* Eight of Mr. *Newton*'s and Mr. *Gregory*'s Series, amongſt which were Mr. *Newton*'s two Series above-mention'd for finding the Arc whoſe Sine is given, and the Sine whoſe Arc is given; and Mr. *Gregory*'s two Series above mentioned for finding the Arc whoſe Tangent is given, and the Tangent whoſe Arc is given. And Mr. *Leibnitz* in his Anſwer, dated *May* 20. 1675. acknowledged the Receipt of this Letter in theſe Words. *Literas tuas multa fruge Algebraica refertas accepi, pro quibus tibi & doctiſſimo* Collinio *gratias ago. Cum nunc præter ordinarias curas Mechanicis imprimis negotiis diſtrahar, non potui examinare Series quas miſiſtis ac cum meis comparare. Ubi fecero perſcribam tibi ſententiam meam: nam aliquot jam anni ſunt quod inveni meas via quadam ſic ſatis ſingulari.*

But yet Mr. *Leibnitz* never took any further notice of his having received theſe Series, nor how his own differed from them, nor ever produced any other Series then thoſe which he received from Mr *Oldenburgh*, or numeral Series deduced from them in particular Caſes. And what he did with Mr. *Gregory*'s Series for finding the Arc whoſe Tangent is given, he has told us in the *Acta Eruditorum menſis Aprilis* 1691. pag. 178. *Jam anno* 1675, ſaith he, *compoſitum habebam opuſculum Quadraturæ Arithmeticæ ab amicis ab illo tempore lectum*, &c. By a Theoreme for tranſmuting of Figures, like thoſe of Dr. *Barrow* and Mr. *Gregory*, he had now found a Demonſtration of this Series, and this was the Subject of his *Opuſculum*. But he ſtill wanted a Demonſtration of the reſt: and meeting with a Pretence to ask for what he wanted, he wrote to Mr. *Oldenburg* the following Letter, dated at *Paris May* 12. 1676.

Cum Georgius Mohr *Danus nobis attulerit communicatam ſibi à Doctiſſimo* Collinio *veſtro expreſſionem rationis inter arcum & ſinum per infinitas Series ſequentes; poſito ſinu* x, *arcu* z, *radio* 1,

$$z = x + \tfrac{1}{6}x^3 + \tfrac{3}{40}x^5 + \tfrac{5}{112}x^7 + \tfrac{35}{1152}x^9 + \&c.$$

$$x = z - \tfrac{1}{6}z^3 + \tfrac{1}{120}z^5 - \tfrac{1}{5040}z^7 + \tfrac{1}{362880}z^9 - \&c.$$

Hæc, INQUAM, cum nobis attulerit ille, quæ mihi valde ingenioſa videntur, & poſterior imprimis Series elegantiam quandam ſingularem habeat: ideo rem gratam mihi feceris, Vir clariſſime, ſi demonſtrationem tranſmiſeris. Habebis viciſſim mea ab his longe diverſa circa hanc rem meditata, de quibus jam aliquot abhinc annis ad te perſcripſiſſe credo, demonſtratione tamen non addita, quam nunc polio. Oro ut Clariſſimo Collinio *multam a me ſalutem dicas: is facile tibi materiam ſuppeditabit ſatisfaciendi deſiderio meo.* Here, by the Word *INQUAM*, one would think that he had never ſeen theſe two Series before, and that his *diverſa circa hanc rem meditata* were ſomething elſe than one of the Series which he had received from

Mr. *Oldenburgh* the Year before, and a Demonſtration thereof which he was now poliſhing, to make the Preſent an acceptable Recompence for Mr. *Newton*'s Method.

Upon the Receipt of this Letter Mr. *Oldenburg* and Mr. *Collins* wrote preſſingly to Mr. *Newton*, deſiring that he himſelf would deſcribe his own Method, to be communicated to Mr. *Leibnitz*. Whereupon Mr. *Newton* wrote his Letter, dated *June* 13. 1676, deſcribing therein the Method of Series, as he had done before in the Compendium above-mentioned; but with this Difference: Here he deſcribed at large the Reduction of the Dignity of a Binomial into a Series, and only touched upon the Reduction by Diviſion and Extraction of affected Roots: There he deſcribed at large the Reduction of Fractions and Radicals into Series by Diviſion and Extraction of Roots, and only ſet down the two firſt Terms of the Series into which the Dignity of a Binomial might be reduced. And among the Examples in this Letter, there were Series for finding the Number whoſe Logarithm is given, and for finding the Verſed Sine whoſe Arc is given: This Letter was ſent to *Paris*, *June* 26. 1676. together with a MS. drawn up by Mr. *Collins*, containing Extracts of Mr. *James Gregory*'s Letters.

For Mr. *Gregory* died near the End of the Year 1675; and Mr. *Collins*, at the Requeſt of Mr *Leibnitz* and ſome other of the Academy of Sciences, drew up Extracts of his Letters, and the Collection is ſtill extant in the Hand Writing of Mr. *Collins* with this Title; *Extracts of Mr.* Gregory'*s Letters, to be lent to Mr.* Leibnitz *to peruſe, who is deſired to return the ſame to you.* And that they were ſent is affirmed by Mr. *Collins* in his Letter to Mr. *David Gregory* the Brother of the Deceas'd, dated *Auguſt* 11. 1676. and appears further by the Anſwers of Mr. *Leibnitz* and Mr. *Tſchurnhauſe* concerning them.

The Anſwer of Mr. *Leibnitz* directed to Mr *Oldenburgh* and dated *Auguſt* 27. 1676, begins thus; *Literæ tuæ die* Julii 26. *datæ plura ac memorabiliora circa rem Analyticam continent quam* *multa volumina ſpiſsâ de his rebus edita. Quare tibi pariter ac clariſſimis viris* Newtono *ac* Collinio *gratias ago, qui nos participes tot meditationum egregiarum eſſe voluiſtis.* And towards the End of the Letter, after he had done with the Contents of Mr. *Newton*'s Letter, he proceeds thus. *Ad alia tuarum Literarum venio quæ doctiſſimus* Collinius *communicare gravatus non eſt. Vellem adjeciſſet appropinquationis* Gregorianæ *linearis demonſtrationem. Fuit enim his certe ſtudiis promovendis aptiſſimus.* And the Anſwer of Mr. *Tſchurnhauſe*, dated *Sept.* 1. 1676, after he had done with Mr. *Newton*'s Letter about Series, concludes thus. *Similia porro quæ in hac re præſtitit eximius ille Geometra* Gregorius *memoranda certe ſunt. Et quidem optimè famæ ipſius conſulturi, qui ipſius relicta Manuſcripta luci publicæ ut exponantur operam navabunt.* In the firſt Part of this Letter, where Mr. *Tſchurnhauſe* ſpeaks of Mr. *Newton*'s Series, he ſaith that he looked over them curſorily, to ſee if he could find the Series of Mr. *Leibnitz* for ſquaring the Circle or Hyperbola. If he had ſearched for it in the Extracts of *Gregory*'s Letters he might have found it in the Letter of *Febr.* 15. 1671. abovementioned. For the MS. of thoſe Extracts with that Letter therein is ſtill extant in the Hand-Writing of Mr. *Collins.*

And tho' Mr. *Leibnitz* had now received this Series twice from Mr. *Oldenburgh*, yet in his Letter of *Auguſt*. 27. 1676. he ſent it back to him by way of Recompence for Mr. *Newton*'s Method, pretending that he had communicated it to his Friends at *Paris* three Years before or above; that is, two Years before he received it in Mr. *Oldenburgh*'s Letter of *April* 15. 1675; at which Time he did not know it to be his own, as appears by his Anſwer of *May* 20. 1675 above-mentioned. He might receive this Series at *London*, and communicate it to his Friends at *Paris* above three Years before he ſent it back to Mr. *Oldenburg*: but it doth not appear that he had the Demonſtration thereof ſo early. When he found the Demonſtration, then he compos'd it in his *Opuſculum*, and communicated that alſo to his Friends; and he himſelf has

told

told us that this was in the Year 1675. However, it lies upon him to prove that he had this Series before he received it from Mr. *Oldenburgh*. For in his Anſwer to Mr. *Oldenburgh* he did not know any of the Series then ſent him to be his own; and concealed from the Gentlemen at *Paris* his having received it from Mr. *Oldenburgh* with ſeveral other Series, and his having ſeen a Copy of the Letter in which Mr. *Gregory* had ſent it to Mr. *Collins* in the Beginning of the Year 1671.

In the ſame Letter of *Auguſt* 27. 1676, after Mr. *Leibnitz* had deſcribed his Quadrature of the Circle and Equilateral Hyperbola, he added: *Viciſſim ex ſeriebus regreſſuum pro Hyperbola hanc inveni. Si ſit numerus aliquis unitate minor* $1-m$, *ejuſque logarithmus Hyperbolicus* l. *Erit* $m = \frac{l}{1} - \frac{l^2}{1\times2} + \frac{l^3}{1\times2\times3} - \frac{l^4}{1\times2\times3\times4} + \&c.$ *Si numerus ſit major unitate, ut* $1+n$, *tunc pro eo inveniendo mihi etiam prodiit Regula quæ in* Newtoni *Epiſtola expreſſa eſt: ſcilicet erit* $n = \frac{l}{1} + \frac{l^2}{1\times2} + \frac{l^3}{1\times2\times3} + \frac{l^4}{1\times2\times3\times4} + \&c.$ —— *Quod regreſſum ex arcubus attinet, incideram ego directe in Regulam quæ ex dato arcu ſinum complementi exhibet. Nempe ſinus complementi* $= 1 - \frac{a^2}{1\times2} + \frac{a^4}{1\times2\times3\times4} - \&c.$ *Sed poſtea quoque deprehendi ex ea illam nobis communicatam pro inveniendo ſinu recto, qui eſt* $\frac{a}{1} - \frac{a^3}{1\times2\times3} + \frac{a^5}{1\times2\times3\times4\times5} - \&c.$ *poſſe demonſtrari.* Thus Mr. *Leibnitz* put in his Claim for the Co-invention of theſe four Series, tho' the Method of finding them was ſent him at his own Requeſt, and he did not yet underſtand it. For in this ſame Letter of *Auguſt* 27 1676. he deſired Mr. *Newton* to explain it further. His Words are. *Sed deſideraverim ut Clariſſimus* Newtonus *nonnulla quoque amplius explicet; ut originem Theorematis quod initio ponit: Item modum quo quantitates* p, q, r, *in ſuis Operationibus invenit: Ac denique quomodo in Methodo regreſſuum ſe gerat, ut cum ex Logarithmo quærit Numerum. Neque enim explicat quomodo id ex methodo ſua derivetur.* He pretended to have found two Series for the Number whoſe Logarithm was given, and yet in the ſame Letter deſired Mr. *Newton* to explain to him the Method of finding thoſe very two Series.

When Mr. *Newton* had received this Letter, he wrote back that all the ſaid four Series had been communicated by him to Mr. *Leibnitz*; the two firſt being one and the ſame Series in which the Letter *l* was put for the Logarithm with its Sign + or −; and the third being the Exceſs of the Radius above the verſed Sine, for which a Series had been ſent to him. Whereupon Mr. *Leibnitz* deſiſted from his Claim. Mr. *Newton* alſo in the ſame Letter dated *Octob.* 24. 1676. further explained his Methods of Regreſſion, as Mr. *Leibnitz* had deſired. And Mr. *Leibnitz* in his Letter of *June* 21. 1677. deſired a further Explication: but ſoon after, upon reading Mr. *Newton*'s Letter a ſecond time, wrote back *July* 12. 1677. that he now underſtood what he wanted; and found by his old Papers that he had formerly uſed one of Mr. *Newton*'s Methods of Regreſſion, but in the Example which he had then by chance made uſe of, there being produced nothing elegant, he had, out of his uſual Impatience, neglected to uſe it any further. He had therefore ſeveral direct Series, and by conſequence a Method of finding them, before he invented and forgot the inverſe Method. And if he had ſearched his old Papers diligently, he might have found this Method alſo there; but having forgot his own Methods he wrote for Mr. *Newton*'s.

When Mr. *Newton* in his Letter dated *June* 13. 1676. had explained his Method of Series, he added: *Ex his videre eſt quantum fines Analyſeos per hujuſmodi infinitas æquationes ampliantur: quippe quæ earum beneficio ad omnia pene dixerim problemata (ſi numeralia* Diophanti *& ſimilia excipias) ſeſe extendit. Non tamen omnino univerſalis evadit, niſi per ulteriores quaſdam Methodos eliciendi Series infinitas. Sunt enim quædam Problemata in quibus non licet ad Series infinitas per Diviſionem vel Extractionem radicum ſimplicium affectarumve pervenire. Sed quomodo in iſtis caſibus procedendum ſit jam non vacat dicere; ut neque alia quædam tradere, quæ circa Reductionem infinitarum Serierum* *infinitas, ubi rei natura tulerit, excogitavi. Nam parcius ſcribo, quod hæ ſpeculationes diu mihi faſtidio eſſe cœperunt; adeo ut ab iiſdem jam per quinque fere annos abſtinuerim.* To this Mr. *Leibnitz* in his Letter of *Auguſt* 27. 676. anſwered: *Quod dicere videmini pleraſque difficultates (exceptis Problematibus* Diophantæis*) ad ſeries Infinitas reduci; id mihi non videtur. Sunt enim multa uſque adeo mira & implexa ut neque ab æquationibus pendeant neque ex Quadraturis. Qualia ſunt (ex multis aliis) Problemata methodi Tangentium inverſæ.* And Mr. *Newton* in his Letter of *Octob.* 24. 1676, replied: *Ubi dixi omnia pene Problemata ſolubilia exiſtere; volui de iis præſertim intelligi circa quæ Mathematici ſe hactenus occuparunt vel ſaltem in quibus Ratiocinia Mathematica locum aliquem obtinere poſſunt. Nam alia ſane adeo perplexis conditionibus implicata excogitare liceat, ut non ſatis comprehendere valeamus: & multo minus tantarum computationum onus ſuſtinere quod iſta requirerent. Attamen ne nimium dixiſſe videar, inverſa de Tangentibus Problemata ſunt in poteſtate, aliaque illis difficiliora. Ad quæ ſolvenda uſus ſum duplici methodo, una concinniori, altera generaliori. Utramque viſum eſt impræſentia literis tranſpoſitis conſignare, ne propter alios idem obtinentes, inſtitutum in aliquibus mutare cogerer.* 5 a cc d æ 10 e ff h, &c. id eſt, *Una methodus conſiſtit in extractione fluentis quantitatis ex æquatione ſimul involvente fluxionem ejus altera tantum in aſſumptione ſeriei pro quantitate qualibet incognita, ex qua cætera commode derivari poſſunt; & in collatione terminorum homologorum æquationis reſultantis ad eruendos terminos aſſumptæ ſeriei.* By Mr. *Newton*'s two Letters, its certain that he had then (or rather above five Years before) found out the Reduction of Problems to fluxional Equations and converging Series: and by the Anſwer of Mr. *Leibnitz* to the firſt of thoſe Letters, its as certain that he had not then found out the Reduction of Problems either to differential Equations or to converging Series.

And the ſame is manifeſt alſo by what Mr. *Leibnitz* wrote in the *Acta Eruditorum, Anno* 1691, concerning this Matter.

Jam anno 1675, ſaith he, *compoſitum habebam opuſculum Quadraturæ Arithmeticæ ab amicis ab illo tempore lectum, ſed quod, materia ſub manibus creſcente, limare ad Editionem non vacavit, poſtquam aliæ occupationes ſupervenere; præſertim cum nunc prolixius exponere vulgari more quæ Analyſis noſtra paucis exhibet, non ſatis operæ pretium videatur.* This Quadrature compoſed *vulgari more* he began to communicate at *Paris* in the Year 1675. The next Year he was poliſhing the Demonſtration thereof, to ſend it to Mr. *Oldenburgh* in Recompence for Mr. *Newton*'s Method, as he wrote to him *May* 12. 1676; and accordingly in his Letter of *Auguſt* 27. 1676. he ſent it compoſed and poliſhed *vulgari more.* The Winter following he returned into *Germany*, by *England* and *Holland*, to enter upon publick Buſineſs, and had no longer any Leiſure to fit it for the Preſs, nor thought it afterwards worth his while to explain thoſe Things prolixly in the vulgar manner which his new *Analyſis* exhibited in ſhort. He found out this new *Analyſis* therefore after his Return into *Germany*, and by conſequence not before the Year 1677.

The ſame is further manifeſt by the following Conſideration. Dr. *Barrow* publiſhed his *Method of Tangents* in the Year 1670. Mr. *Newton* in his Letter dated *December* 10. 1672. communicated his *Method of Tangents* to Mr. *Collins*, and added: *Hoc eſt unum particulare vel Corollarium potius Methodi generalis, quæ extendit ſe citra moleſtum ullum calculum, non modo ad ducendum Tangentes ad quaſvis Curvas ſive Geometricas ſive Mechanicas, vel quomodocunque rectas Lineas aliaſve Curvas reſpicientes; verum etiam ad reſolvendum alia abſtruſiora Problematum genera de Curvitatibus, Areis, Longitudinibus, Centris Gravitatis Curvarum,* &c. *Neque (quemadmodum* Huddenii *methodus de* Maximis *&* Minimis) *ad ſolas reſtringitur æquationes illas, quæ quantitatibus ſurdis ſunt immunes. Hanc methodum intertexui alteri iſti qua Æquationum Exegeſin inſtituo, reducendo eas ad ſeries infinitas.* Mr. *Sluſius* ſent his Method of Tangents to Mr. *Oldenburgh Jan.* 17. 167$\frac{2}{3}$, and the ſame was ſoon after publiſhed in the *Tranſactions* It proved to be the ſame with that of Mr *Newton.* It was founded upon three *Lemmas,* the firſt of which was this, *Differentia duarum dignitatum ejuſdem gradus applicata ad differentiam laterum dat partes ſingulares gradus inferioris ex binomio laterum, ut* $\frac{y^3 - x^3}{y - x} = yy + yx + xx$, that is, in the Notation of Mr. *Leibnitz* $\frac{dy^3}{dy} = 3yy$. A Copy of Mr *Newton*'s Letter of *Decemb.* 10. 1672 was ſent to Mr. *Leibnitz* by Mr *Oldenburg* amongſt the Papers of Mr *James Gregory,* at the ſame time with Mr. *Newton*'s Letter of *June* 13. 1676. And Mr *Newton* having deſcribed in theſe two Letters that he had a very general *Analyſis,* conſiſting partly of the Method of converging Series, partly of another Method, by which he applied thoſe Series to the Solution of almoſt all Problems (except perhaps ſome numeral ones like thoſe of *Diophantus*) and found the Tangents, Areas, Lengths, ſolid Contents, Centers of Gravity, and Curvities of Curves, and curvilinear Figures Geometrical or Mechanical, without ſticking at Surds; and that the Method of Tangents of *Sluſius* was but a Branch or Corollary of this other Method: Mr. *Leibnitz* in his returning Home through *Holland,* was meditating upon the Improvement of the Method of *Sluſius.* For in a Letter to Mr. *Oldenburgh,* dated from *Amſterdam Nov.* $\frac{18}{28}$ 1676, he wrote thus *Methodus Tangentium à* Sluſio *publicata nondum rei faſtigium tenet. Poteſt aliquid amplius præſtari in eo genere quod maximi foret uſus ad omnis generis Problemata: etiam ad meam (ſine extractionibus) Æquationum ad ſeries reductionem. Nimirum poſſet brevis quædam calculari circa Tangentes Tabula, eouſque continuanda donec progreſſio Tabulæ apparet; ut eam ſcilicet quiſque quouſque libuerit ſine calculo continuare poſſit.* This was the Improvement of the Method of *Sluſius* into a general Method, which Mr. *Leibnitz* was then thinking upon, and by his Words, *Poteſt aliquid amplius præſtari in eo genere quod maximi foret uſus ad omnis generis Problemata*, it ſeems to be the only Improvement which he had then in his Mind for extending the Method to all ſorts of Problems. The Improvement by the differential Calculus was not yet in his Mind, but muſt be referred to the next Year.

Mr. *Newton* in his next Letter, dated *Octob.* 24. 1676, mentioned the *Analyſis* communicated by Dr. *Barrow* to Mr. *Collins* in the Year 1669, and alſo another Tract written in 1671. about converging Series, and about the other Method by which Tangents were drawn after the Method of *Sluſius*, and *Maxima* and *Minima* were determined, and the Quadrature of Curves was made more eaſy, and this without ſticking at Radicals, and by which Series were invented which brake off and gave the Quadrature of Curves in finite Equations when it might be. And the Foundation of theſe Operations he comprehended in this Sentence expreſt enigmatically as above. *Data æquatione fluentes quotcunque quantitates involvente fluxiones invenire, & vice verſa.* Which puts it paſt all Diſpute that he had invented the Method of Fluxions before that time. And if other things in that Letter be conſidered, it will appear that he had then brought it to great Perfection, and made it exceeding general; the Propoſitions in his Book of Quadratures, and the Methods of converging Series and of drawing a Curve Line through any Number of given Points, being then known to him. For when the Method of Fluxions proceeds not in finite Equations, he reduces the Equations into converging Series by the binomial Theoreme, and by the Extraction of Fluents out of Equations involving or not involving their Fluxions. And when finite Equations are wanting, he deduces converging Series from the Conditions of the Probleme, by aſſuming the Terms of the Series gradually, and determining them by thoſe Conditions. And when Fluents are to be derived from Fluxions, and the Law of the Fluxions is wanting, he finds that Law *quam proxime*, by drawing a Parabolick Line through any Number of given Points. And by theſe Improvements Mr. *Newton* had in thoſe Days made his Method of Fluxions much more univerſal than the Differential Method of Mr. *Leibnitz* is at preſent.

This Letter of Mr. *Newton*'s, dated *Octob.* 24. 1676, came to the Hands of Mr. *Leibnitz* in the End of the Winter or Beginning of the Spring following; and Mr. *Leibnitz* ſoon after, *viz.* in a Letter dated *June* 21. 1677, wrote back: *Clariſſimi* Sluſii *methodum Tangentium nondum eſſe abſolutam Celeberrimo* Newtono *aſſentior. Et jam à multo tempore rem Tangentium generalius tractavi, ſcilicet per differentias Ordinatarum.* —— *Hinc nominando*, in poſterum, dy *differentiam duarum proximarum* y *&c.* Here Mr. *Leibnitz* began firſt to propoſe his Differential Method, and there is not the leaſt Evidence that he knew it before the Receipt of Mr. *Newton*'s laſt Letter. He ſaith indeed, *Jam à multo tempore rem Tangentium generalius tractavi, ſcilicet per differentias Ordinatarum*: and ſo he affirmed in other Letters, that he had invented ſeveral converging Series direct and inverſe before he had the Method of inventing them; and had forgot an inverſe Method of Series before he knew what uſe to make of it. But no Man is a Witneſs in his own Cauſe. A Judge would be very unjuſt, and act contrary to the Laws of all Nations, who ſhould admit any Man to be a Witneſs in his own Cauſe. And therefore it lies upon Mr. *Leibnitz* to prove that he found out this Method long before the Receipt of Mr. *Newton*'s Letters. And if he cannot prove this, the Queſtion, Who was the firſt Inventor of the Method, is decided.

The Marquiſs *De l'Hoſpital* (a Perſon of very great Candour) in the Preface to his Book *De Analyſi quantitatum infinitè* '*parvarum*, publiſhed *A. C.* 1696. tells us, that a little after 'the Publication of the Method of Tangents of *Des Cartes*, ' Mr. *Fermat* found alſo a Method, which *Des Cartes* himſelf 'at length allowed to be, for the moſt part, more ſimple than 'his own. But it was not yet ſo ſimple as Mr. *Barrow* after'wards made it, by conſidering more nearly the nature of Po'lygons, which offers naturally to the Mind a little Triangle, 'compos'd of a Particle of the Curve lying between two Or'dinates infinitely near one another, and of the Difference of 'theſe two Ordinates, and of that of the two correſpondent '*Abſciſſa*'s. And this Triangle is like that which ought to be 'made by the Tangent, the Ordinate, and the Sub-tangent: 'ſo that by one ſimple Analogy, this laſt Method ſaves all 'the Calculation which was requiſite either in the Method of '*Des Cartes*, or in this ſame Method before. Mr. *Barrow* 'ſtopt not here, he invented alſo a ſort of Calculation proper 'for this Method. But it was neceſſary in this as well as in 'that of *Des Cartes*, to take away Fractions and Radicals for 'making it uſeful. Upon the Defect of this Calculus, that of 'the celebrated Mr. *Leibnitz* was introduced, and this learned 'Geometer began where Mr. *Barrow* and others left off. This 'his *Calculus* led into Regions hitherto unknown, and there 'made Diſcoveries which aſtoniſhed the moſt able Mathema'ticians of *Europe*,' &c. Thus far the Marquiſs. He had not ſeen Mr. *Newton*'s *Analyſis*, nor his Letters of *Decem.* 10. 1672. *June* 13. 1676, and *Octob.* 24. 1676: and ſo not knowing that Mr. *Newton* had done all this and ſignified it to Mr. *Leibnitz*, he reckoned that Mr. *Leibnitz* began where Mr. *Barrow* left off, and by teaching how to apply Mr. *Barrow*'s Method without ſticking at Fractions and Surds, had enlarged the Method wonderfully. And Mr. *James Bernoulli*, in the *Acta Eruditorum* of *January* 1691 *pag.* 14. writes thus: *Qui calculum* Barrovianum *(quem in Lectionibus ſuis Geometricis adumbravit Auctor, cujuſque Specimina ſunt tota illa Propoſitionum inibi contentarum farrago,) intellexerit,* [calculum] *alterum à Domino* Leibnitio *inventum, ignorare vix poterit; utpote qui in priori illo fundatus eſt, & niſi forte in Differentialium notatione & operationis aliquo compendio ab eo non differt.*

Now Dr. *Barrow*, in his Method of Tangents, draws two Ordinates indefinitely near to one another, and puts the Letter *a* for the Difference of the Ordinates, and the Letter *e* for the Difference of the *Abſciſſa*'s, and for drawing the Tangent gives theſe Three Rules 1. *Inter computandum*, ſaith he, *omnes abjicio terminos in quibus ipſarum* a *vel* e *poteſtas habeatur, vel in quibus ipſæ ducuntur in ſe. Etenim iſti termini nihil valebunt.* 2. *Poſt æquationem conſtitutam omnes abjicio terminos literis conſtantes quantitates notas ſeu determinatas ſignificantibus, aut in quibus non habentur* a *vel* e. *Etenim illi termini ſemper ad unam æquationis partem adducti nihilum adæquabunt.* 3. *Pro* a *Ordinatam, & pro* e *Subtangentem ſubſtituo. Hinc demum Subtangentis quantitas dignoſcetur.* Thus far Dr. *Barrow*.

And Mr. *Leibnitz* in his Letter of *June* 21. 1677 above-mentioned, wherein he firſt began to propoſe his Differential Method, has followed this Method of Tangents exactly, excepting that he has changed the Letters *a* and *e* of Dr. *Barrow* into *dx* and *dy*. For in the Example which he there gives, he draws two parallel Lines and ſets all the Terms below the under Line, in which *dx* and *dy* are (ſeverally or jointly) of more than one Dimenſion, and all the Terms above the upper Line, in which *dx* and *dy* are wanting, and for the Reaſons given by Dr. *Barrow*, makes all theſe Terms vaniſh. And by the Terms in which *dx* and *dy* are but of one Dimenſion, and which he ſets between the two Lines, he determines the Proportion of the Subtangent to the Ordinate. Well therefore did the Marquiſs *de l'Hoſpital* obſerve that where Dr. *Barrow* left off Mr. *Leibnitz* began: for their Methods of Tangents are exactly the ſame.

But Mr. *Leibnitz* adds this Improvement of the Method, that the Concluſion of this Calculus is coincident with the Rule of *Sluſius*, and ſhews how that Rule preſently occurs to any one who underſtands this Method. For Mr. *Newton* had repreſented in his Letters, that this Rule was a Corollary of his general Method.

And whereas Mr. *Newton* had ſaid that his Method in drawing of Tangents, and determining *Maxima* and *Minima*, &c. proceeded without ſticking at Surds: Mr. *Leibnitz* in the next Place, ſhews how this Method of Tangents may be improved ſo as not to ſtick at Surds or Fractions, and then adds: *Arbitror quæ celare voluit* Newtonus *de Tangentibus ducendis ab his non abludere. Quod addit, ex hoc eodem fundamento Quadraturas quoque reddi faciliores me in hac ſententia confirmat; nimirum ſemper figuræ illæ ſunt quadrabiles quæ ſunt ad æquationem differentialem.* By which Words, compared with the preceding Calculation, its manifeſt that Mr. *Leibnitz* at this time underſtood that Mr. *Newton* had a Method which would do all theſe things, and had been examining whether Dr. *Barrow*'s Differential Method of Tangents might not be extended to the ſame Performances.

In *November* 1684 Mr. *Leibnitz* publiſhed the Elements of this Differential Method in the *Acta Eruditorum*, and illuſtrated it with Examples of drawing Tangents and determining *Maxima* and *Minima*, and then added. *Et hæc quidem initia ſunt Geometriæ cujuſdam multo ſublimioris, ad difficillima & pulcherrima quæque etiam miſtæ* Matheſeos *Problemata pertingentis, quæ ſine calculo differentiali* AUT SIMILI *non temere quiſquam pari facilitate tractabit.* The Words AUT SIMILI plainly relate to Mr. *Newton*'s Method. And the whole Sentence contains nothing more than what Mr. *Newton* had affirmed of his general Method in his Letters of 1672 and 1676.

And in the *Acta Eruditorum* of *June* 1686, *pag.* 297. Mr. *Leibnitz* added: *Malo autem* dx *& ſimilia adhibere quam literas pro illis, quia iſtud* dx *eſt modificatio quædam ipſius* x, &c. He knew that in this Method he might have uſed Letters with Dr. *Barrow*, but choſe rather to uſe the new Symbols *dx* and *dy*, though there is nothing which can be done by theſe Symbols, but may be done by ſingle Letters with more brevity.

The next Year Mr. *Newton's Principia Philoſophiæ* came abroad, a Book full of ſuch Problemes as Mr. *Leibnitz* had called *difficillima & pulcherrima etiam miſtæ Matheſeos problemata, quæ ſine calculo differentiali aut* SIMILI *non temere quiſquam pari facilitate tractabit.* And the Marqueſs *de L'Hoſpital* has repreſented this Book *preſque tout de ce calcul*; compoſed almoſt wholly of this Calculus. And Mr. *Leibnitz* himſelf in a Letter to Mr. *Newton*, dated from *Hannover, March* $\frac{7}{17}$ 1693 and ſtill extant in his own Hand-writing, and upon a late Occaſion communicated to the Royal Society, acknowledged the ſame thing in theſe Words: *Mirifice ampliaveras Geometriam tuis Seriebus, ſed edito* Principiorum *opere oſtendiſti patere tibi etiam quæ Analyſi receptæ non ſubſunt. Conatus ſum ego quoque, notis commodis adhibitis quæ differentias & ſummas exhibeant, Geometriam illam quam Tranſcendentem appello Analyſi quodammodo ſubjicere, nec res male proceſſit*; And again in his Anſwer to Mr. *Fatio*, printed in the *Acta Eruditorum* of *May* 1700. *pag.* 203. *lin.* 21. he acknowledged the ſame thing. In the ſecond Lemma of the ſecond Book of theſe *Principles*, the Elements of this Calculus are demonſtrated ſynthetically, and at the End of the Lemma there is a Scholium in theſe Words. *In Literis quæ mihi cum Geometra peritiſſimo* G. G. Leibnitio *annis abhinc decem intercedebant, cum ſignificarem me compotem eſſe methodi determinandi Maximas & Minimas, ducendi Tangentes & ſimilia peragendi, quæ in terminis ſurdis æque ac in rationalibus procederet; & literis tranſpoſitis hanc ſententiam involventibus* [Data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, & vice verſa] *eandem celarem: reſcripſit Vir clariſſimus ſe quoque in ejuſmodi methodum incidiſſe, & methodum ſuam communicavit à mea vix abludentem præterquam in verborum & notarum formulis. Utriuſque fundamentum continetur in hoc Lemmate.* In thoſe Letters, and in another dated *Decem.* 10. 1672, a Copy of which, at that time, was ſent to Mr. *Leibnitz* by Mr. *Oldenburgh*, as is mentioned above, Mr. *Newton* had ſo far explained his Method, that it was not difficult for

Mr. *Leibnitz*, by the Help of Dr. *Barrow*'s Method of Tangents, to collect it from thoſe Letters. And its certain, by the Arguments above-mentioned, that he did not know it before the writing of thoſe Letters.

Dr. *Wallis* had received Copies of Mr. *Newton*'s two Letters of *June* 13. and *Octob.* 24. 1676 from Mr. *Oldenburgh*, and publiſhed ſeveral things out of them in his *Algebra*, printed in *Engliſh* 1683, and in Latin 1693; and ſoon after had Intimation from *Holland* to print the Letters entire, becauſe Mr. *Newton*'s Notions of Fluxions paſſed there with Applauſe by the Name of the Differential Method of Mr. *Leibnitz*. And thereupon he took notice of this Matter in the Preface to the firſt Volume of his Works publiſhed *A. C.* 1695. And in a Letter to Mr. *Leibnitz* dated *Decemb.* 1. 1696, he gave the Account of it. *Cum Præfationis (præfigendæ) poſtremum folium erat ſub prælo, ejuſque typos jam poſuerant Typothetæ; me monuit amicus quidam (harum rerum gnarus) qui peregre fuerat, tum talem methodum in Belgio prædicari, tum illam cum* Newtoni *methodo Fluxionum quaſi coincidere. Quod fecit ut (tranſlatis typis jam poſitis) id monitum interſeruerim.* And in a Letter dated *April* 10. 1695, and lately communicated to the Royal-Society, he wrote thus about it. *I wiſh you would print the two large Letters of* June *and* Auguſt [he means *June* and *October*] 1676. *I had intimation from* Holland, *as deſired there by your Friends, that ſomewhat of that kind were done; becauſe your Notions (of Fluxions) paſs there with great Applauſe by the Name of* Leibnitz's Calculus Differentialis. *I had*[*] *this intimation when all but part of the Preface to this Volume was printed off; ſo that I could only inſert (while the Preſs ſtay'd) that ſhort Intimation thereof which you there find. You are not ſo kind to your Reputation (and that of the Nation) as you might be, when you let things of worth lye by you ſo long, till others carry away*

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[*] Extat hæc Epiſtola in tertio volumine operum *Walliſii*.

the Reputation that is due to you. I have endeavoured to do you Juſtice in that Point, and am now ſorry that I did not print thoſe two Letters verbatim.

The ſhort Intimation of this Matter, which Dr. *Wallis* inſerted into the ſaid Preface, was in theſe Words. *In ſecundo Volumine (inter alia) habetur* Newtoni *Methodus de Fluxionibus (ut ille loquitur) conſimilis naturæ cum* Leibnitii (*ut hic loquitur) Calculo Differentiali (quod qui utramque methodum contulerit ſatis animadvertat, ut ut ſub loquendi formulis diverſis) quam ego deſcripſi* (*Algebræ* cap. 91. &c. *præſertim* cap. 95) *ex binis* Newtoni *Literis, aut earum alteris,* Junii 13. *&* Octob. 24. 1676 *ad* Oldenburgum *datis, cum* Leibnitio *tum communicandis (iiſdem fere verbis, ſaltem leviter mutatis, quæ in illis literis habentur,) ubi* METHODUM HANC *LEIBNITIO* EXPONIT, *tum ante* DECEM ANNOS *nedum plures* [id eſt, anno 1666 vel 1665] *ab ipſo excogitatam. Quod moneo, nequis cauſetur de hoc Calculo Differentiali nihil à nobis dictum eſſe.*

Hereupon the Editors of the *Acta Lipſienſia,* the next Year in *June,* in the Style of Mr. *Leibnitz,* in giving an Account of theſe two firſt Volumes of Dr. *Wallis,* took notice of this Clauſe of the Doctor's Preface, and complained, not of his ſaying that Mr. *Newton* in his two Letters above-mentioned explained to Mr *Leibnitz* the Method of Fluxions found by him Ten Years before or above; but that while the Doctor mentioned the Differential Calculus, and ſaid that he did it *nequis cauſetur de calculo differentiali nihil ab ipſo dictum fuiſſe,* he did not tell the Reader that Mr. *Leibnitz* had this Calculus at that time when thoſe Letters paſſed between him and Mr. *Newton,* by means of Mr. *Oldenburgh.* And, in ſeveral Letters which followed hereupon, between Mr. *Leibnitz* and Dr. *Wallis*, concerning this Matter, Mr. *Leibnitz* denied not that Mr. *Newton* had the Method Ten Years before the writing of thoſe Letters, as Dr. *Wallis* had affirmed; pretended not that he himſelf had the Method ſo early; brought no Proof that he had it before the Year

1677; no other Proof beſides the Conceſſion of Mr. *Newton* that he had it ſo early; affirmed not that he had it earlier; commended Mr. *Newton* for his Candour in this Matter; allowed that the Methods agreed in the main, and ſaid that he therefore uſed to call them by the common Name of his *Infiniteſimal Analyſis*; repreſented, that as the Methods of *Vieta* and *Cartes* were called by the common Name of *Analyſis Specioſa*, and yet differed in ſome things; ſo perhaps the Methods of Mr *Newton* and himſelf might differ in ſome things, and challenged to himſelf only thoſe things wherein, as he conceived, they might differ, naming the Notation, the differential Equations and the Exponential Equations. But in his Letter of *June* 21. 1677 he reckon'd differential Equations common to Mr. *Newton* and himſelf.

This was the State of the Diſpute between Dr. *Wallis* and Mr *Leibnitz* at that time And Four years after, when Mr. *Fatio* ſuggeſted that Mr. *Leibnitz*, the ſecond Inventor of this Calculus, might borrow ſomething from Mr. *Newton*, the oldeſt Inventor by many Years: Mr. *Leibnitz* in his Anſwer, publiſhed in the *Acta Eruditorum* of *May* 1700, allowed that Mr. *Newton* had found the Method apart, and did not deny that Mr. *Newton* was the oldeſt Inventor by many Years, nor aſſerted any thing more to himſelf, than that he alſo had found the Method apart, or without the Aſſiſtance of Mr. *Newton*, and pretended that when he firſt publiſhed it, he knew not that Mr. *Newton* had found any thing more of it than the Method of Tangents. And in making this Defence he added: *Quam* [methodum] *ante Dominum* Newtonum *&* Me *nullus quod ſciam Geometra habuit; uti ante hunc maximi nominis Geometram N E M O ſpecimine publice dato ſe habere probavit, ante Dominos* Bernoullios *&* Me *nullus communicavit*. Hitherto therefore Mr. *Leibnitz* did not pretend to be the *firſt* Inventor. He did not begin to put in ſuch a Claim till after the Death of Dr. *Wallis*, the laſt of the old Men who were acquainted with what had paſſed between the *Engliſh* and Mr. *Leibnitz*

Forty Years ago. The Doctor died in *October A. C.* 1703, and Mr. *Leibnitz* began not to put in this new Claim before *January* 1705.

Mr. *Newton* publiſhed his *Treatiſe of Quadratures* in the Year 1704. This Treatiſe was written long before, many things being cited out of it in his Letters of *Octob.* 24. and *Novemb.* 8. 1676. It relates to the Method of Fluxions, and that it might not be taken for a new Piece, Mr. *Newton* repeated what Dr. *Wallis* had publiſhed Nine Years before without being then contradicted, namely, that this Method was invented by Degrees in the Years 1665 and 1666. Hereupon the Editors of the *Acta Lipſienſia* in *January* 1705. in the Style of Mr. *Leibnitz,* in giving an Account of this Book, repreſented that Mr. *Leibnitz* was the firſt Inventor of the Method, and that Mr. *Newton* had ſubſtituted Fluxions for Differences. And this Accuſation gave a Beginning to this preſent Controverſy.

For Mr. *Keill*, in an Epiſtle publiſhed in the *Philoſophical Tranſactions* for *Sept.* and *Octob.* 1708, retorted the Accuſation, ſaying: *Fluxionum Arithmeticam ſine omni dubio primus invenit D.* Newtonus, *ut cuilibet ejus Epiſtolas à* Walliſio *editas legenti facile conſtabit. Eadem tamen Arithmetica poſtea mutatis nomine & notationis modo à Domino* Leibnitio *in* Actis Eruditorum *edita eſt.*

Before Mr. *Newton* ſaw what had been publiſhed in the *Acta Leipſica,* he expreſs'd himſelf offended at the printing of this Paragraph of Mr. *Keill*'s Letter, leaſt it ſhould create a Controverſy. And Mr. *Leibnitz,* underſtanding it in a ſtronger Senſe than Mr. *Keill* intended it, complain'd of it as a Calumny, in a Letter to Dr. *Sloane* dated *March* 4. 1711 *N. S.* and moved that the Royal-Society would cauſe Mr. *Keill* to make a publick Recantation. Mr. *Keill* choſe rather to explain and defend what he had written; and Mr. *Newton,* upon being ſhewed the Accuſation in the *Acta Lipſica,* gave him leave to do ſo. And Mr. *Leibnitz* in a ſecond Letter to Dr. *Sloane,* dated *Decem.* 29. 1711, inſtead of making good his Accuſation, as he was bound to do that it might not be deem'd a Calumny, inſiſted only upon his own Candour, as if it would be Injuſtice to queſtion it; and refus'd to tell how he came by the Method; and ſaid that the *Acta Lipſica* had given every Man his due, and that he had concealed the Invention above Nine Years, (he ſhould have ſaid Seven Years) that No body might pretend (he means that Mr. *Newton* might not pretend) to have been before him in it; and called Mr. *Keill* a Novice unacquainted with things paſt, and one that acted without Authority from Mr. *Newton*, and a clamorous Man who deſerved to be ſilenced, and deſired that Mr. *Newton* himſelf would give his Opinion in the Matter. He knew that Mr. *Keill* affirmed nothing more than what Dr. *Wallis* had publiſhed thirteen Years before, without being then contradicted. He knew that Mr. *Newton* had given his Opinion in this matter in the Introduction to his Book of *Quadratures*, publiſhed before this Controverſy began: but Dr. *Wallis* was dead; the Mathematicians which remained in *England* were Novices; Mr. *Leibnitz* may Queſtion any Man's Candour without Injuſtice, and Mr. *Newton* muſt now retract what he had publiſhed or not be quiet.

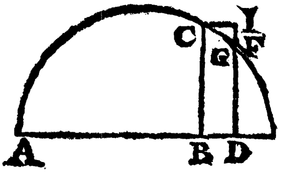
The Royal-Society therefore, having as much Authority over Mr. *Leibnitz* as over Mr. *Keill*, and being now twice preſſed by Mr. *Leibnitz* to interpoſe, and ſeeing no reaſon to condemn or cenſure Mr. *Keill* without enquiring into the matter; and that neither Mr. *Newton* nor Mr. *Leibnitz* (the only Perſons alive who knew and remembred any thing of what had paſſed in theſe matters Forty Years ago) could be Witneſſes for or againſt Mr. *Keill*; appointed a numerous Committee to ſearch old Letters and Papers, and report their Opinion upon what they found; and ordered the Letters and Papers, with the Report of their Committee to be publiſhed. And by theſe Letters and Papers it appear'd to them, that Mr. *Newton* had the Method in or before the Year 1669, and it did not appear to them that Mr. *Leibnitz* had it before the Year 1677.

For making himſelf the firſt Inventor of the Differential Method, he has repreſented that Mr. *Newton* at firſt uſed the Letter o in the vulgar manner for the given Increment of x, which deſtroys the Advantages of the Differential Method; but after the writing of his Principles, changed o into $\dot{x}$, ſubſtituting $\dot{x}$ for dx. It lies upon him to prove that Mr. *Newton* ever changed o into $\dot{x}$, or uſed $\dot{x}$ for dx, or left off the Uſe of the Letter o. Mr. *Newton* uſed the Letter o in his *Analyſis* written in or before the Years 1669, and in his Book of *Quadratures*, and in his *Principia Philoſophiæ*, and ſtill uſes it in the very ſame Senſe as at firſt. In his Book of Quadratures he uſed it in conjunction with the Symbol $\dot{x}$, and therefore did not uſe that Symbol in its Room. Theſe Symbols o and $\dot{x}$ are put for things of a different kind. The one is a Moment, the other a Fluxion or Velocity as has been explained above. When the Letter x is put for a Quantity which flows uniformly, the Symbol $\dot{x}$ is an Unit, and the Letter o a Moment, and $\dot{x}o$ and dx ſignify the ſame Moment. Prickt Letters never ſignify Moments, unleſs when they are multiplied by the Moment o either expreſt or underſtood to make them infinitely little, and then the Rectangles are put for Moments.

Mr. *Newton* doth not place his Method in Forms of Symbols, nor confine himſelf to any particular Sort of Symbols for Fluents and Fluxions. Where he puts the Areas of Curves for Fluents, he frequently puts the Ordinates for Fluxions, and denotes the Fluxions by the Symbols of the Ordinates, as in his *Analyſis*. Where he puts Lines for Fluents, he puts any Symbols for the Velocities of the Points which deſcribe the Lines, that is, for the firſt Fluxions; and any other Symbols for the Increaſe of thoſe Velocities, that is, for the ſecond Fluxions, as is frequently done in his *Principia Philoſophiæ*. And where he puts the Letters x, y, z for Fluents, he denotes their Fluxions, either by other Letters as p, q, r; or by the ſame Letters in other Forms as X, Y, Z or $\dot{x}, \dot{y}, \dot{z}$; or by any Lines as *D E*, *FG*, *H I*, conſidered as their Exponents. And this is evident by his Book of *Quadratures*, where he repreſents Fluxions by prickt Letters in the firſt Propoſition, by Ordinates of Curves in the laſt Propoſition, and by other Symbols, in explaining the Method and illuſtrating it with Examples, in the Introduction. Mr. *Leibnitz* hath no Symbols of Fluxions in his Method, and therefore Mr. *Newton*'s Symbols of Fluxions are the oldeſt in the kind. Mr. *Leibnitz* began to uſe the Symbols of Moments or Differences *dx*, *dy*, *dz* in the Year 1677. Mr. *Newton* repreſented Moments by the Rectangles under the Fluxions and the Moment *o*, when he wrote his *Analyſis*, which was at leaſt Forty Six Years ago. Mr. *Leibnitz* has uſed the Symbols *ſx*, *ſy*, *ſz* for the Sums of Ordinates ever ſince the Year 1686; Mr. *Newton* repreſented the ſame thing in his *Analyſis*, by inſcribing the Ordinate in a Square or Rectangle. All Mr. *Newton*'s Symbols are the oldeſt in their ſeveral Kinds by many Years.

And whereas it has been repreſented that the uſe of the Letter *o* is vulgar, and deſtroys the Advantages of the Differential Method: on the contrary, the Method of Fluxions, as uſed by Mr. *Newton*, has all the Advantages of ths Differential, and ſome others. It is more elegant, becauſe in his Calculus there is but one infinitely little Quantity repreſented by a Symbol, the Symbol *o*. We have no Ideas of infinitely little Quantities, and therefore Mr. *Newton* introduced Fluxions into his Method, that it might proceed by finite Quantities as much as poſſible. It is more Natural and Geometrical, becauſe founded upon the *primæ quantitatum naſcentium rationes*, which have a Being in Geometry, whilſt *Indiviſibles*, upon which the Differential Method is founded, have no Being either in Geometry or in Nature. There are *rationes primæ quantitatum naſcentium*, but not *quantitates primæ naſcentes*. Nature generates Quantities by continual Flux or Increaſe; and the ancient Geometers admitted ſuch a Generation of Areas and Solids, when they drew one Line into another by local Motion to generate an Area, and the Area into a Line by local Motion to generate a Solid. But the ſumming up of Indiviſibles to compoſe an Area or Solid was never yet admitted into Geometry. Mr. *Newton*'s Method is alſo of greater Uſe and Certainty, being adapted either to the ready finding out of a Propoſition by ſuch Approximations as will create no Error in the Concluſion, or to the demonſtrating it exactly: Mr. *Leibnitz*'s is only for finding it out. When the Work ſucceeds not in finite Equations Mr. *Newton* has recourſe to converging Series, and thereby his Method becomes incomparably more univerſal than that of Mr. *Leibnitz*, which is confin'd to finite Equations: for he has no Share in the Method of infinite Series. Some Years after the Method of Series was invented, Mr. *Leibnitz* invented a Propoſition for tranſmuting curvilinear Figures into other curvilinear Figures of equal Areas, in order to ſquare them by converging Series: but the Methods of ſquaring thoſe other Figures by ſuch Series were not his. By the help of the new *Analyſis* Mr. *Newton* found out moſt of the Propoſitions in his *Principia Philoſophiæ*: but becauſe the Ancients for making things certain admitted nothing into Geometry before it was demonſtrated ſynthetically, he demonſtrated the Propoſitions ſynthetically, that the Syſteme of the Heavens might be founded upon good Geometry. And this makes it now difficult for unskilful Men to ſee the Analyſis by which thoſe Propoſitions were found out.

It has been repreſented that Mr. *Newton*, in the Scholium at the End of his Book of Quadratures, has put the third, fourth, and fifth Terms of a converging Series reſpectively equal to the ſecond, third, and fourth Differences of the firſt Term, and therefore did not then underſtand the Method of ſecond, third, and fourth Differences. But in the firſt Propoſition of that Book he ſhewed how to find the firſt, ſecond, third and following Fluxions *in infinitum*; and therefore when he wrote that Book, which was before the Year 1676, he did underſtand the Method of all the Fluxions, and by conſequence of all the Differences. And if he did not underſtand it when he added that Scholium to the End of the Book, which was in the Year 1704, it muſt have been becauſe he had then forgot it. And ſo the Queſtion is only whether he had forgot the Method of ſecond and third Differences before the Year 1704.



In the Tenth Propoſition of the ſecond Book of his *Principia Philoſophiæ*, in deſcribing ſome of the Uſes of the Terms of a converging Series for ſolving of Problemes, he tells us that if the firſt Term of the Series repreſents the Ordinate *BC* of any Curve Line *ACG*, and *CBDI* be a Parallelogram infinitely narrow, whoſe Side *DI* cuts the Curve in *G* and its Tangent *CF* in *F*, the ſecond Term of the Series will repreſent the Line *IF*, and the third Term the Line *FG*. Now the Line *FG* is but half the ſecond Difference of the Ordinate: and therefore Mr. *Newton* when he wrote his *Principia*, put the third Term of the Series equal to half of the ſecond Difference of the firſt Term, and by conſequence had not then forgotten the Method of ſecond Differences.

In writing that Book, he had frequent occaſion to conſider the Increaſe or Decreaſe of the Velocities with which Quantities are generated, and argues right about it. That Increaſe or Decreaſe is the ſecond Fluxion of the Quantity, and therefore he had not then forgotten the Method of ſecond Fluxions.

In the Year 1692, Mr. *Newton*, at the Requeſt of Dr. *Wallis*, ſent to him a Copy of the firſt Propoſition of the Book of Quadratures, with Examples thereof in firſt, ſecond and third Fluxions: as you may ſee in the ſecond Volume of the Doctor's Works, *pag*. 391, 392, 393 and 396. And therefore he had not then forgotten the Method of ſecond Fluxions.

Nor is it likely, that in the Year 1704, when he added the aforeſaid Scholium to the End of the Book of Quadratures, he had forgotten not only the firſt Propoſition of that Book, but alſo the laſt Propoſition upon which that Scholium was written. If the Word [*ut*], which in that Scholium may have been accidentally omitted between the Words [*erit*] and [*ejus*,] be reſtor'd, that Scholium will agree with the two Propoſitions and with the reſt of his Writings, and the Objection will vaniſh.

Thus much concerning the Nature and Hiſtory of theſe Methods, it will not be amiſs to make ſome Obſervations thereupon.

In the *Commercium Epiſtolicum*, mention is made of three Tracts written by Mr. *Leibnitz*, after a Copy of Mr. *Newton*'s *Principia Philoſophiæ* had been ſent to *Hannover* for him; and after he had ſeen an Account of that Book publiſhed in the *Acta Eruditorum* for *January* and *February* 1689. And in thoſe Tracts the principal Propoſitions of that Book are compoſed in a new manner, and claimed by Mr. *Leibnitz* as if he had found them himſelf before the publiſhing of the ſaid Book. But Mr. *Leibnitz* cannot be a Witneſs in his own Cauſe. It lies upon him either to prove that he found them before Mr. *Newton*, or to quit his claim.

In the laſt of thoſe three Tracts, the 20*th* Propoſition (which is the chief of Mr. *Newton*'s Propoſitions) is made a Corollary of the 19*th* Propoſition, and the 19*th* Propoſition has an erroneous Demonſtration adapted to it. It lies upon him either to ſatisfy the World that the Demonſtration is not erroneous, or to acknowledge that he did not find that and the 20*th* Propoſition thereby, but tried to adapt a Demonſtration to Mr. *Newton*'s Propoſition to make it his own. For he repreſents in his 20*th* Propoſition that he knew not how Mr. *Newton* came by it, and by conſequence that he found it himſelf without the Aſſiſtance of Mr. *Newton*.

By the Errors in the 15*th* and 19*th* Propoſition of the third Tract, Dr. *Keill* hath ſhewed that when Mr. *Leibnitz* wrote theſe three Tracts, he did not well underſtand the Ways of working in ſecond Differences. And this is further manifeſt by the *10th*, *11th*, and *12th* Propoſitions of this third Tract. For theſe he lays down as the Foundation of his infiniteſimal Analyſis in arguing about centrifugal Forces, and propoſes the firſt of them with relation to the Center of Curvity of the Orb, but uſes this Propoſition in the two next, with Relation to the Center of Circulation. And by confounding theſe two Centers with one another in the fundamental Propoſitions upon which he grounds this *Calculus*, he erred in the Superſtructure, and for want of Skill in ſecond and third Differences, was not able to extricate himſelf from the Errors. And this is further confirmed by the ſixth Article of the ſecond Tract. For that Article is erroneous, and the Error ariſes from his not knowing how to argue well about ſecond and third Differences. When therefore he wrote thoſe Tracts he was but a Learner, and this he ought in candour to acknowledge.

It ſeems therefore that as he learnt the Differential Method by means of Mr. *Newton*'s aforeſaid three Letters compared with Dr. *Barrow*'s Method of Tangents; ſo Ten Years after, when Mr. *Newton*'s *Principia Philoſophiæ* came abroad, he improved his Knowledge in theſe Matters, by trying to extend this Method to the Principal Propoſitions in that Book, and by this means compoſed the ſaid three Tracts. For the Propoſitions contained in them (Errors and Trifles excepted) are Mr. *Newton*'s (or eaſy Corollaries from them) being publiſhed by him in other Forms of Words before: And yet Mr. *Leibnitz* publiſhed them as invented by himſelf long before they were publiſhed by Mr. *Newton*. For in the End of the firſt Tract, he repreſents that he invented them all before Mr. *Newton*'s *Principia Philoſophiæ* came abroad, and ſome of them before he left *Paris*, that is before *October* 1676. And the ſecond Tract he concludes with theſe Words: *Multa ex his deduci poſſent praxi accommodata, ſed nobis* *his nunc fundamenta Geometrica jeciſſe ſuffecerit, in quibus maxima conſiſtebat difficultas. Et fortaſſis attente conſideranti vias quaſdam novas ſatis antea impeditas aperuiſſe videbimur. Omnia autem reſpondent noſtræ Analyſi Infinitorum, hoc eſt calculo Summarum & Differentiarum(cujus elementa quædam in his Actis dedimus) communibus quoad licuit verbis hic expreſſo.* He pretends here that the *Fundamenta Geometrica in quibus maxima conſiſtebat difficultas* were firſt laid by himſelf in this very Tract, and that he himſelf had in this very Tract opened *vias quaſdam novas ſatis antea impeditas.* And yet Mr. *Newton*'s *Principia Philoſophiæ* came abroad almoſt two Years before, and gave occaſion to the Writing of this Tract, and was written *communibus quoad licuit verbis,* and contains all theſe Principles and all theſe new Ways. And Mr. *Leibnitz,* when he publiſhed that Tract, knew all this, and therefore ought then to have acknowledged that Mr. *Newton* was the firſt who laid the *Fundamenta Geometrica in quibus maxima conſiſtebat Difficultas,* and opened the *vias novas ſatis antea impeditas.* In his Anſwer to Mr. *Fatio* he acknowledged all this, ſaying *Quam* [methodum] *ante Dominum* Newtonum *& me nullus quod ſciam Geometra habuit; uti ante hunc maximi nominis Geometram, NEMO SPECIMINE PUBLICE DATO ſe habere PROBAVIT.* And what he then acknowledged he ought in Candour and Honour to acknowledge ſtill upon all Occaſions.

Mr. *Leibnitz* in his Letter of *May* 28. 1697, wrote thus to Dr. *Wallis. Methodum Fluxionum profundiſſimi* Newtoni *cognatam eſſe methodo meæ differentiali non tantum animadverti poſtquam opus ejus* [Principiorum ſcilicet] *& tuum prodiit; ſed etiam profeſſus ſum in Actis Eruditorum, & alias quoque monui. Id enim candori meo convenire judicavi, non minus quam ipſius merito. Itaque communi nomine deſignare ſoleo Analyſeos infiniteſimalis; quæ latius quam Tetragoniſtica patet. Interim quemadmodum &* Vietæa *&* Carteſiana *methodus Analyſeos ſpecioſæ nomine venit; diſcrimina tamen nonnulla ſuperſunt: ita fortaſſe*

&

& Newtoniana & Mea *differunt in nonnullis.* Here alſo Mr. *Leibnitz* allows that when Mr *Newton*'s Principles of Philoſophy came abroad, he underſtood thereby the Affinity that there was between the Methods, and therefore called them both by the common Name of the infiniteſimal Method, and thought himſelf bound in candour to acknowledge this Affinity: and there is ſtill the ſame Obligation upon him in point of Candour. And beſides this Acknowledgment, he here gives the Preference to Mr. *Newton*'s Method in Antiquity. For he repreſents that as the vulgar Analyſis in *Species* was invented by *Vieta*, and augmented by *Cartes*, which made ſome Differences between their Methods: ſo Mr. *Newton*'s Method and his own might differ in ſome things. And then he goes on to enumerate the Differences by which he had improved Mr. *Newton*'s Method as we mentioned above. And this Subordination of his Method to Mr. *Newton*'s, which he then acknowledged to Dr. *Wallis*, he ought ſtill to acknowledge.

In enumerating the Differences or Improvements which he had added to Mr. *Newton*'s Method; he names in the ſecond Place Differential Equations: but the Letters which paſſed between them in the Year 1676, do ſhow that Mr. *Newton* had ſuch Equations at that time, and Mr. *Leibnitz* had them not. He names in the third Place Exponential Equations: but theſe Equations are owing to his Correſpondence with the *Engliſh*. Dr. *Wallis*, in the Interpolation of Series, conſidered Fract and Negative Indices of Dignities. Mr. *Newton* introduced into his Analytical Computations, the Fract, Surd, Negative and Indefinitive Indices of Dignities; and in his Letter of *October* 24. 1676, repreſented to Mr. *Leibnitz* that his Method extended to the Reſolution of affected Equations involving Dignities whoſe Indices were Fract or Surd. Mr. *Leibnitz* in his Anſwer dated *June* 21. 1677, mutually deſired Mr. *Newton* to tell him what he thought of the Reſolution of Equations involving

Dignities whoſe Indices were undetermined, ſuch as were theſe $x^y + y^x = xy$, $x^x + y^y = x + y$ And theſe Equations he now calls Exponential, and repreſents to the World that he was the firſt Inventor thereof, and magnifies the Invention as a great Diſcovery. But he has not yet made a publick Acknowledgment of the Light which Mr. *Newton* gave him into it, nor produced any one Inſtance of the uſe that he has been able to make of it where the Indices of Dignities are Fluents. And ſince he has not yet rejected it with his uſual Impatience for want of ſuch an Inſtance, we have reaſon to expect that he will at length explain its Uſefulneſs to the World.

Mr. *Newton* in his Letter of *October* 24. 1676 wrote that he had two Methods of reſolving the Inverſe Problems of Tangents, and ſuch like difficult ones; one of which conſiſted *in aſſuming a Series for any unknown Quantity from which all the reſt might conveniently be deduced, and in collating the homologous Terms of the reſulting Equation, for determining the Terms of the aſſumed Series.* Mr. *Leibnitz* many Years after publiſhed this Method as his own, claiming to himſelf the firſt Invention thereof. It remains that he either renounce his Claim publickly, or prove that he invented it before Mr. *Newton* wrote his ſaid Letter.

It lies upon him alſo to make a publick Acknowledgment of his Receipt of Mr. *Oldenburgh*'s Letter of *April* 15. 1675, wherein ſeveral converging Series for ſquaring of Curves, and particularly that of Mr. *James Gregory* for finding the Arc by the given Tangent, and thereby ſquaring the Circle, were communicated to him. He acknowledged it privately in his Letter to Mr. *Oldenburg* dated *May* 20. 1675 ſtill extant in his own Hand-writing, and by Mr *Oldenburg* left entred in the Letter-Book of the *Royal-Society*. But he has not yet acknowledged it publickly, as he ought to have done when he publiſhed that Series as his own.

It lies upon him alſo to make a publick Acknowledgment of his having received the Extracts of Mr. *James Gregory*'s Letters, which, at his own Requeſt, were ſent to him at *Paris* in *June* 1676 by Mr. *Oldenburgh* to peruſe: amongſt which was Mr. *James Gregory*'s Letter of *Feb.* 15. 1671, concerning that Series, and Mr. *Newton*'s Letter of *December* 10. 1672 concerning the Method of Fluxions.

And whereas in his Letter of *Decem.* 28. 1675 he wrote to Mr. *Oldenburgh*, that he had communicated that Series above two Years before to his Friends at *Paris*, and had written to him ſometimes about it; and in his Letter of *May* 12. 1676 ſaid to Mr. *Oldenburgh* that he had written to him about that Series ſome Years before; and in his Letter to Mr. *Oldenburgh* dated *Aug.* 27. 1676, that he had communicated that Series to his Friends above three Years before; that is, upon his firſt coming from *London* to *Paris*: He is deſired to tell us how it came to paſs, that when he received Mr. *Oldenburgh*'s Letter of *Apr.* 15. 1675 he did not know that Series to be his own.

In his Letters of *July* 15. and *Octob.* 26. 1674, he tells us of but one Series for the circumference of a Circle, and ſaith that the Method which gave him this Series, gave him alſo a Series for any Arc whoſe Sine was given, tho' the Proportion of the Arc to the whole Circumference be not known. This Method therefore, by the given Sine of 30 Degrees, gave him a Series for the whole Circumference. If he had alſo a Series for the whole Circumference, deduced from the Tangent of 45 Degrees, he is deſired to tell the World what Method he had in thoſe Days, which could give him both thoſe Series. For the Method by the Tranſmutation of Figures will not do it. He is deſired alſo to tell us why in his ſaid Letters he did not mention more Quadratures of the Circle than one.

And if in the Year 1674 he had the Demonſtration of a Series for finding any Arc whoſe Sine is given, he is deſired to tell the World what it was ; and why in his Letter of *May* 12. 1676 he deſired Mr. *Oldenburgh* to procure from Mr. *Collins* the Demonſtration of Mr. *Newton*'s Series for doing the ſame thing ; and wherein his own Series differed from Mr. *Newton*'s. For upon all theſe Conſiderations there is a Suſpicion that Mr. *Newton*'s Series for finding the Arc whoſe Sine is given, was communicated to him in *England*; and that in the Year 1673 he began to communicate it as his own to ſome of his Friends at *Paris*, and the next Year wrote of it as his own in his Letters to Mr. *Oldenburgh*, in order to get the Demonſtration or Method of finding ſuch Series. But the Year following, when Mr. *Oldenburgh* ſent him this Series and the Series of Mr. *Gregory* and Six other Series, he dropt his Pretence to this Series for want of a Demonſtration, and took time to conſider the Series ſent him, and to compare them with his own, as if his Series were others different from thoſe ſent him. And when he had found a Demonſtration of *Gregory*'s Series by a Tranſmutation of Figures, he began to communicate it as his own to his Friends at *Paris*, as he repreſents in the *Acta Eruditorum* for *April* 1691. *pag.* 178, ſaying; *Jam Anno* 1675 *compoſitum habebam opuſculum Quadraturæ Arithmeticæ ab Amicis ab illo tempore lectum*, &c. But the Letter by which he had received this Series from Mr. *Oldenburgh* he concealed from his Friends, and pretended to Mr. *Oldenburgh* that he had this Series a Year or two before the Receipt of that Letter. And the next Year, upon receiving two of Mr. *Newton*'s Series again by one *George Mohr*, he wrote to Mr. *Oldenburgh* in ſuch a manner as if he had never ſeen them before, and upon Pretence of their Novelty, deſired Mr. *Oldenburgh* to procure from Mr. *Collins* Mr. *Newton*'s Method of finding them. If Mr. *Leibnitz* thinks fit to obviate this Suſpicion, he is in the firſt Place to prove that he had Mr. *Gregory*'s Series before he received it from Mr. *Oldenburgh*.

It lies upon him alſo to tell the World what was the Method by which the ſeveral Series of Regreſſion for the Circle and Hyperbola, ſent to him by Mr. *Newton June* 13. 1676, and claimed as his own by his Letter of *Auguſt* 27. following, were found by him before he received them from Mr. *Newton*.

And whereas Mr. *Newton* ſent him, at his own Requeſt, a Method of Regreſſion, which upon the firſt reading he did not know to be his own, nor underſtood it; but ſo ſoon as he underſtood it he claimed as his own, by pretending that he had found it long before, and had forgot it, as he perceived by his old Papers: it lies upon him, in point of Candor and Juſtice, either to prove that he was the firſt Inventor of this Method, or to renounce his Claim to it for preventing future Diſputes.

Mr. *Leibnitz* in his Letter to Mr. *Oldenburgh* dated *Feb.* 3. $167\frac{2}{3}$ claimed a Right to a certain Property of a Series of Numbers Natural, Triangular, Pyramidal, Triangulo-Triangular, *&c.* and to make it his own, repreſented that he wondred that Monſieur *Paſcal*, in his Book entituled *Triangulum Arithmeticum*, ſhould omit it. That Book was publiſhed in the Year 1665, and contains this Property of the Series; and Mr. *Leibnitz* has not yet done him the Juſtice to acknowledge that he did not omit it. It lies upon him therefore in Candor and Juſtice, to renounce his Claim to this Property, and acknowledge Mr. *Paſchal* the firſt Inventor.

He is alſo to renounce all Right to the Differential Method of *Mouton* as ſecond Inventor: for ſecond Inventors have no Right. The ſole Right is in the firſt Inventor until another finds out the ſame thing apart. In which caſe to take away the Right of the firſt Inventor, and divide it between him and that other, would be an Act of Injuſtice.

In his Letter to Dr. *Sloane* dated *Decem.* 29. 1711. he has told us that his Friends know how he came by the Differential Method. It lies upon him, in point of Candor, openly and plainly, and without further Heſitation, to ſatisfy the World how he came by it.

In the ſame Letter he has told us that he had this Method above Nine Years before he publiſhed it, and it follows from thence that he had it in the Year 1675 or before. And yet its certain that he had it not when he wrote his Letter to Mr. *Oldenburgh* dated *Aug.* 27. 1676, wherein he affirmed that Problems of the Inverſe Method of Tangents and many others, could not be reduced to infinite Series, nor to Equations or Quadratures. It lies upon him therefore, in point of Candor, to tell us what he means by pretending to have found the Method before he had found it.

We have ſhewed that Mr. *Leibnitz* in the End of the Year 1676, in returning home from *France* through *England* and *Holland*, was meditating how to improve the Method of *Sluſius* for Tangents, and extend it to all ſorts of Problems, and for this end propoſed the making of a general Table of Tangents; and therefore had not yet found out the true Improvement. But about half a Year after, when he was newly fallen upon the true Improvement, he wrote back; *Clariſſ.* Sluſii *Methodum Tangentium nondum eſſe abſolutam Celeberrimo* Newtono *aſſentior. Et jam A MULTO TEMPORE rem Tangentium generalius tractavi, ſcilicet per differentias Ordinatarum.* Which is as much as to ſay that he had this Improvement long before thoſe Days. It lies upon him, in point of Candor, to make us underſtand that he pretended to this Antiquity of his Invention with ſome other Deſign than to rival and ſupplant Mr. *Newton*, and to make us believe that he had the Differential Method before Mr. *Newton* explained it to him by his Letters of *June* 13. and *Octob.* 24. 1676, and before Mr. *Oldenburgh* ſent him a Copy of Mr. *Newton*'s Letter of *Decem* 10. 1672 concerning it.

The Editors of the *Acta Eruditorum* in *June* 1696, in giving an Account of the two firſt Volumes of the Mathematical Works of Dr. *Wallis*, wrote thus, in the Style of Mr. *Leibnitz*. *Cæterum ipſe* Newtonus, *non minus Candore quam præclaris in rem Mathematicam meritis inſignis, publice &*

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privatim agnovit Leibnitium, *tum cum* (*interveniente celeberrimo Viro* Henrico Oldenburgo *Bremenſi*, *Societatis Regiæ* Anglicanæ *tunc Secretario*) *inter ipſos* (*ejuſdem jam tum Societatis Socios*) *Commercium intercederet, id eſt jam fere ante annos viginti & amplius, Calculum ſuum differentialem, Serieſque infinitas, & pro iis quoque Methodos generales habuiſſe*; *quod* Walliſius *in Præfatione Operum, factæ inter eos communicationis mentionem faciens, præteriit, quoniam de eo fortaſſe non ſatis ipſi conſtabat. Cæterum Differentiarum conſideratio* Leibnitiana, *cujus mentionem facit* Walliſius (*ne quis ſcilicet, ut ipſe ait, cauſeretur de Calculo Differentiali nihil ab ipſo dictum fuiſſe*) *meditationes aperuit, quæ aliunde non æque naſcebantur, &c.* By the Words here cited out of the Preface to the two firſt Volumes of Dr. *Wallis*'s Works, it appears that Mr. *Leibnitz* had ſeen that Part of the Preface, where Mr. *Newton* is ſaid to have explained to him (in the Year 1676) the Method of Fluxions found by him Ten Years before or above. Mr. *Newton* never allowed that Mr. *Leibnitz* had the Differential Method before the Year 1677. And Mr. *Leibnitz* himſelf in the *Acta Eruditorum* for *April* 1691. pag. 178. acknowledged that he found it after he returned home from *Paris* to enter upon Buſineſs, that is, after the Year 1676. And as for his pretended general Method of infinite Series, it is ſo far from being general, that it is of little or no uſe. I do not know that any other Uſe hath been made of it, than to colour over the Pretence of Mr. *Leibnitz* to the Series of Mr. *Gregory* for ſquaring the Circle.

Mr. *Leibnitz*, in his Anſwer to Mr. *Fatio* printed in the *Acta Eruditorum* for the Year 1700. pag. 203. wrote thus. *Ipſe* [Newtonus] *ſcit unus omnium optime, ſatiſque indicavit publice cum ſua* Mathematica Naturæ Principia *publicaret, Anno* 1687, *nova quædam inventa Geometrica, quæ ipſi communia mecum fuere, NEUTRUM LUCI AB ALTERO ACCEPTÆ, ſed meditationibus quemque ſuis debere, & a me decennio ante* [i. e. anno 1677] *expoſita fuiſſe.* In the

Book of *Principles* here referred unto, Mr. *Newton* did not acknowledge that Mr. *Leibnitz* found this Method without receiving Light into it from Mr. *Newton*'s Letters above-mentioned; and Dr. *Wallis* had lately told him the contrary without being then confuted or contradicted. And if Mr. *Leibnitz* had found the Method without the Aſſiſtance of Mr. *Newton*, yet ſecond Inventors have no Right.

Mr. *Leibnitz* in his aforeſaid Anſwer to Mr. *Fatio*, wrote further: *Certe cum elementa Calculi mea edidi anno* 1684, *ne conſtabat quidem mihi aliud de inventis ejus* [ſc. Newtoni] *in hoc genere, quam quod ipſe olim ſignificaverat in literis, poſſe ſe Tangentes invenire non ſublatis irrationalibus, quod* Hugenius *quoque ſe poſſe mihi ſignificavit poſtea, etſi cæterorum ejus Calculi adhuc expers. Sed majora multo conſecutum* Newtonum, *viſo demum libro* Principiorum *ejus, ſatis intellexi.* Here he again acknowledged that the Book of *Principles* gave him great Light into Mr. *Newton*'s Method: and yet he now denies that this Book contains any thing of that Method in it. Here he pretended that before that Book came abroad he knew nothing more of Mr. *Newton*'s Inventions of this kind, than that he had a certain Method of Tangents. and that by that Book he received the firſt Light into Mr. *Newton*'s Method of Fluxions: but in his Letter of *June* 21. 1677 he acknowledged that Mr. *Newton*'s Method extended alſo to Quadratures of curvilinear Figures, and was like his own. His Words are; *Arbitror quæ celare voluit* Newtonus *de Tangentibus ducendis ab his non abludere. Quod addit, ex hoc eodem fundamento Quadraturas quoque reddi faciliores me in ſententia hac confirmat; nimirum ſemper figuræ illæ ſunt quadrabiles quæ ſunt ad æquationem differentialem.*

Mr. *Newton* had in his three Letters above-mentioned (copies of which Mr. *Leibnitz* had received from Mr. *Oldenbergh*) repreſented his Method ſo general, as by the Help of Equations, finite and infinite, to determin *Maxima* and *Minima*, Tangents, Areas, ſolid Contents, Centers of Gravity, Lengths and Curvities of curve Lines and curvilinear Figures, and this without taking away Radicals, and to extend to the like Problems in Curves uſually called Mechanical, and to inverſe Problems of Tangents and others more difficult, and to almoſt all Problems, except perhaps ſome Numeral ones like thoſe of *Diophantus*. And Mr. *Leibnitz* in his Letter of *Aug.* 27 1676, repreſented that he could not believe that Mr. *Newton*'s Method was ſo general. Mr. *Newton* in the Firſt of his three Letters ſet down his Method of Tangents deduced from this general Method, and illuſtrated it with an Example, and ſaid that this Method of Tangents was but a Branch or Corollary of his General Method, and that he took the Method of Tangents of *Sluſius* to be of the ſame kind: and thereupon Mr. *Leibnitz*, in his Return from *Paris* through *England* and *Holland* into *Germany*, was conſidering how to improve the Method of Tangents of *Sluſius*, and extend it to all ſorts of Problems, as we ſhewed above out of his Letters. And in his third Letter Mr. *Newton* illuſtrated his Method with Theorems for Quadratures and Examples thereof. And when he had made ſo large an Explanation of his Method, that Mr. *Leibnitz* had got Light into it, and had in his Letter of *June* 21. 1677 explained how the Method which he was fallen into anſwered to the Deſcription which Mr. *Newton* had given of his Method, in drawing of Tangents giving the Method of *Sluſius*, proceeding without taking away Fractions and Surds, and facilitating Quadratures; for him to tell the *Germans* that in the Year 1684, when he firſt publiſhed his Differential Method, he knew nothing more of Mr. *Newton*'s Invention, than that he had a certain Method of Tangents, is very extraordinary and wants an Explanation.

At that time he explained nothing more concerning his own Method, than how to draw Tangents and determin *Maxima* and *Minima* without taking away Fractions or Surds. He certainly knew that Mr. *Newton*'s Method would do all this, and therefore ought in Candor to have acknowledged it. After he had thus far explained his own Method, he added that what he had there laid down were the Principles of a much ſublimer Geometry, reaching to the moſt difficult and valuable Problems, which were ſcarce to be reſolved without the Differential Calculus, *AUT SIMILI*, or another like it. What he meant by the Words *AUT SIMILI* was impoſſible for the *Germans* to underſtand without an Interpreter. He ought to have done Mr. *Newton* juſtice in plain intelligible Language, and told the *Germans* whoſe was the *Methodus SIMILIS*, and of what Extent and Antiquity it was, according to the Notices he had received from *England*; and to have acknowledged that his own Method was not ſo ancient. This would have prevented Diſputes, and nothing leſs than this could fully deſerve the Name of Candor and Juſtice. But afterwards, in his Anſwer to Mr. *Fatio*, to tell the *Germans* that in the Year 1684, when he firſt publiſhed the Elements of his Calculus, he knew nothing of a *Methodus SIMILIS*, nothing of any other Method than for drawing Tangents, was very ſtrange and wants an Explanation.

It lies upon him alſo to ſatisfy the World why, in his Anſwer to Dr. *Wallis* and Mr. *Fatio*, who had publiſhed that Mr. *Newton* was the oldeſt Inventor of that Method by many Years, he did not put in his Claim of being the oldeſt Inventor thereof, but ſtaid till the old Mathematicians were dead, and then complained of the new Mathematicians as Novices; attacked Mr. *Newton* himſelf, and declined to contend with any Body elſe, notwithſtanding that Mr. *Newton* in his Letter of *Octob.* 24. 1676 had told him, that for the ſake of Quiet, he had Five Years before that time laid aſide his Deſign of publiſhing what he had then written on this Subject, and has ever ſince induſtriouſly avoided all Diſputes about Philoſophical and Mathematical Subjects, and all Correſpondence by Letters about thoſe Matters, as tending to Diſputes;

putes; and for the ſame Reaſon has forborn to complain of Mr. *Leibnitz*, untill it was ſhewed him that he ſtood accuſed of Plagiary in the *Acta Lipſiæ*, and that what Mr *Keill* had publiſhed was only in his Defence from the Guilt of that Crime.

It has been ſaid the Royal-Society gave judgment againſt Mr. *Leibnitz* without hearing both Parties. But this is a Miſtake. They have not yet given judgment in the Matter. Mr. *Leibnitz* indeed deſired the Royal-Society to condemn Mr. *Keill* without hearing both Parties; and by the ſame ſort of Juſtice they might have condemned Mr. *Leibnitz* without hearing both Parties; for they have an equal Authority over them both. And when Mr. *Leibnitz* declined to make good his Charge againſt Mr. *Keill*, the Royal-Society might in juſtice have cenſured him for not making it good. But they only appointed a Committee to ſearch out and examin ſuch old Letters and Papers as were ſtill extant about theſe Matters, and report their Opinion how the Matter ſtood according to thoſe Letters and Papers. They were not appointed to examin Mr. *Leibnitz* or Mr. *Keill*, but only to report what they found in the ancient Letters and Papers: and he that compares their Report therewith will find it juſt. The Committee was numerous and ſkilful and compoſed of Gentlemen of ſeveral Nations, and the Society are ſatisfied in their Fidelity in examining the Hands and other Circumſtances, and in printing what they found in the ancient Letters and Papers ſo examined, without adding, omitting or altering any thing in favour of either Party. And the Letters and Papers are by order of the Royal-Society preſerved, that they may be conſulted and compared with the *Commercium Epiſtolicum*, whenever it ſhall be deſired by Perſons of Note. And in the mean time I take the Liberty to acquaint him, that by taxing the Royal-Society with Injuſtice in giving Sentence againſt him without hearing both Parties, he has tranſgreſſed one of their Statutes which makes it Expulſion to defame them.

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The Philoſophy which Mr. *Newton* in his *Principles* and *Optiques* has purſued is Experimental; and it is not the Buſineſs of Experimental Philoſophy to teach the Cauſes of things any further than they can be proved by Experiments. We are not to fill this Philoſophy with Opinions which cannot be proved by Phænomena. In this Philoſophy Hypotheſes have no place, unleſs as Conjectures or Queſtions propoſed to be examined by Experiments. For this Reaſon Mr. *Newton* in his Optiques diſtinguiſhed thoſe things which were made certain by Experiments from thoſe things which remained uncertain, and which he therefore propoſed in the End of his Optiques in the Form of Queries. For this Reaſon, in the Preface to his *Principles*, when he had mention'd the Motions of the Planets, Comets, Moon and Sea as deduced in this Book from Gravity, he added: *Utinam cætera Naturæ Phænomena ex Principiis Mechanicis eodem argumentandi genere derivare liceret. Nam multa me movent ut nonnihil ſuſpicer ea omnia ex viribus quibuſdam pendere poſſe, quibus corporum particulæ per cauſas nondum cognitas vel in ſe mutuo impelluntur & ſecundum figuras regulares cohærent, vel ab invicem fugantur & recedunt: quibus viribus ignotis Philoſophi hactenus Naturam fruſtra tentarunt.* And in the End of this Book in the ſecond Edition, he ſaid that for want of a ſufficient Number of Experiments, he forbore to deſcribe the Laws of the Actions of the Spirit or Agent by which this Attraction is performed. And for the ſame Reaſon he is ſilent about the Cauſe of Gravity, there occurring no Experiments or Phænomena by which he might prove what was the Cauſe thereof. And this he hath abundantly declared in his *Principles*, near the Beginning thereof, in theſe Words; *Virium cauſas & ſedes Phyſicas jam non expendo.* And a little after: *Voces Attractionis, Impulſus, vel Propenſionis cujuſcunque in centrum indifferenter & pro ſe mutuo promiſcue uſurpo, has Vires non Phyſice ſed Mathematice tantum conſiderando. Unde caveat Lector ne per hujuſmodi voces cogitet me* *ſpeciem vel modum actionis, cauſamve aut rationem phyſicam alicubi definire, vel Centris (quæ ſunt puncta Mathematica) vires verè & phyſicè tribuere, ſi forte aut Centra trahere aut vires Centrorum eſſe dixero.* And in the End of his Opticks: *Qua cauſa efficiente hæ attractiones* [ſc. gravitas, viſque magnetica & electrica] *peragantur, hic non inquiro. Quam ego Attractionem appello, fieri ſane poteſt ut ea efficiatur impulſu vel alio aliquo modo nobis incognito. Hanc vocem Attractionis ita hic accipi velim ut in univerſum ſolummodo vim aliquam ſignificare intelligatur qua corpora ad ſe mutuo tendant, cuicunque demum cauſæ attribuenda ſit illa vis. Nam ex Phænomenis Naturæ illud nos prius edoctos oportet quænam corpora ſe invicem attrahant, & quænam ſint leges & proprietates iſtius attractionis, quam in id inquirere par ſit quanam efficiente cauſa peragatur attractio.* And a little after he mentions the ſame Attractions as Forces which by Phænomena appear to have a Being in Nature, tho' their Cauſes be not yet known; and diſtinguiſhes them from occult Qualities which are ſuppoſed to flow from the ſpecifick Forms of things. And in the Scholium at the End of his *Principles*, after he had mentioned the Properties of Gravity, he added: *Rationem vero harum Gravitatis proprietatum ex Phænomenis nondum potui deducere, & Hypotheſes non fingo. Quicquid enim ex Phænomenis non deducitur Hypotheſis vocanda eſt; & Hypotheſes ſeu Metaphyſicæ ſeu Phyſicæ, ſeu Qualitatum occultarum, ſeu Mechanicæ, in Philoſophia experimentali locum non habent. —— ſatis eſt quod Gravitas revera exiſtat & agat ſecundum leges à nobis expoſitas, & ad Corporum cæleſtium & Maris noſtri motus omnes ſufficiat.* And after all this, one would wonder that Mr. *Newton* ſhould be reflected upon for not explaining the Cauſes of Gravity and other Attractions by Hypotheſes; as if it were a Crime to content himſelf with Certainties and let Uncertainties alone. And yet the Editors of the *Acta Eruditorum, (a)* have told the World that Mr. *Newton* denies that the cauſe of Gravity is Mechanical, and that if the Spirit or Agent by which Electrical Attraction is performed, be not the *Ether* or *ſubtile Matter* of *Cartes*, it is leſs valuable than an Hypotheſis, and perhaps may be the Hylarchic Principle of Dr. *Henry Moor*: and Mr. *Leibnitz (b)* hath accuſed him of making Gravity a natural or eſſential Property of Bodies, and an occult Quality and Miracle. And by this ſort of Railery they are perſwading the *Germans* that Mr. *Newton* wants Judgment, and was not able to invent the Infiniteſimal Method.

(a) *Anno* 1714, *menſe Martio*, p. 141. 142. (b) *In tractatu de Bonitate Dei & in Epiſtolis ad D.* Hartſoeker *& alibi.*

It muſt be allowed that theſe two Gentlemen differ very much in Philoſophy. The one proceeds upon the Evidence ariſing from Experiments and Phænomena, and ſtops where ſuch Evidence is wanting; the other is taken up with Hypotheſes, and propounds them, not to be examined by Experiments, but to be believed without Examination. The one for want of Experiments to decide the Queſtion, doth not affirm whether the Cauſe of Gravity be Mechanical or not Mechanical: the other that it is a perpetual Miracle if it be not Mechanical. The one (by way of Enquiry) attributes it to the Power of the Creator that the leaſt Particles of Matter are hard: the other attributes the Hardneſs of Matter to conſpiring Motions, and calls it a perpetual Miracle if the Cauſe of this Hardneſs be other than Mechanical. The one doth not affirm that animal Motion in Man is purely mechanical: the other teaches that it is purely mechanical, the Soul or Mind (according to the Hypotheſis of an *Harmonia Præſtabilita*) never acting upon the Body ſo as to alter or influence its Motions. The one teaches that God (the God in whom we live and move and have our Being) is Omnipreſent; but not as a Soul of the World: the other that he is not the Soul of the World, but *INTELLIGENTIA SUPRAMUNDANA*, an Intelligence above the Bounds of the World; whence it ſeems to follow that he cannot do any thing within the Bounds of the World, unleſs by an incredible Miracle. The one teaches that Philoſophers are to argue from Phænomena and *Experiments* to the Cauſes thereof, and thence to the Cauſes of thoſe Cauſes, and ſo on till we come to the firſt Cauſe: the other that all the Actions of the firſt Cauſe are Miracles, and all the Laws impreſt on Nature by the Will of God are perpetual Miracles and occult Qualities, and therefore not to be conſidered in Philoſophy. But muſt the conſtant and univerſal Laws of Nature, if derived from the Power of God or the Action of a Cauſe not yet known to us, be called Miracles and occult Qualities, that is to ſay, *Wonders* and *Abſurdities*? Muſt all the Arguments for a God taken from the Phænomena of Nature be exploded by *new hard Names*? And muſt Experimental Philoſophy be exploded as *miraculous* and *abſurd*, becauſe it aſſerts nothing more than can be proved by Experiments, and we cannot yet prove by Experiments that all the Phænomena in Nature can be ſolved by meer Mechanical Cauſes? Certainly theſe things deſerve to be better conſidered.

ERRATA. *Pag.* 199. *lin.* 14. *put an* Aſterisk (*) *after the Word* Letter.